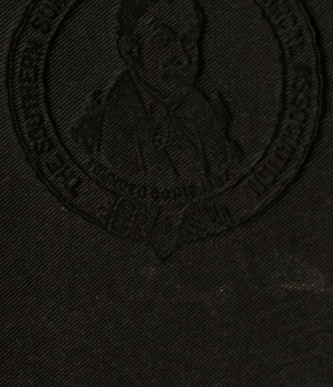

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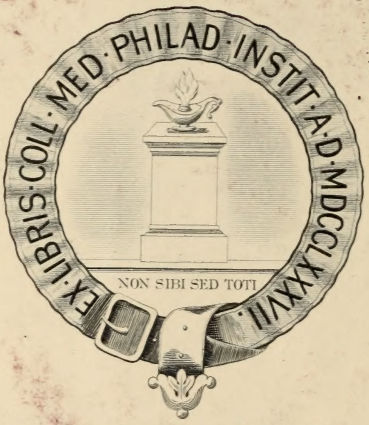


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# TRANSACTIONS

OF THE

# SOUTHERN SURGICAL AND GYNECOLOGICAL ASSOCIATION

VOLUME XIX.

*NINETEENTH SESSION,*

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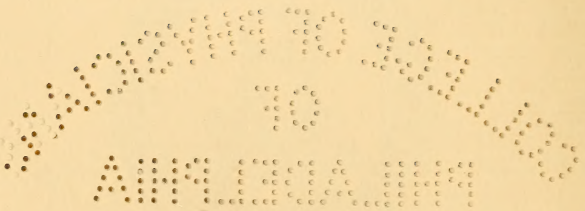

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## OFFICERS FOR 1906-1907.

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VICE-PRESIDENTS.

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HUBERT A. ROYSTER, Raleigh, N. C.

SECRETARY.

WILLIAM D. HAGGARD, Nashville, Tenn.

TREASURER.

CHARLES M. ROSSER, Dallas, Texas.

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RICHARD DOUGLAS, Nashville, Tenn.
F. W. PARHAM, New Orleans, La.
LEWIS S. McMURTRY, Louisville, Ky.
GEORGE H. NOBLE, Atlanta, Ga.

CHAIRMAN COMMITTEE OF ARRANGEMENTS.

E. DENEGRE MARTIN, New Orleans, La.

# LIST OF OFFICERS

## FROM THE ORGANIZATION TO THE PRESENT TIME.

| | *President.* | *Vice-Presidents.* | *Secretary.* | *Treasurer.* |
|---|---|---|---|---|
| 1888. | *W. D. Haggard. | R. D. Webb.<br>J. W. Sears. | *W. E. B. Davis. | Hardin P. Cochrane. |
| 1889. | *Hunter H. McGuire. | W. O. Roberts.<br>*Bedford Brown. | *W. E. B. Davis. | Hardin P. Cochrane. |
| 1890. | *George J. Engelmann. | *Berthold E. Hadra.<br>Duncan Eve. | *W. E. B. Davis. | Hardin P. Cochrane. |
| 1891. | Lewis S. McMurtry. | *J. McFadden Gaston.<br>J. T. Wilson. | *W. E. B. Davis. | Hardin P. Cochrane. |
| 1892. | *J. McFadden Gaston. | *Cornelius Kollock.<br>George Ben Johnston. | *W. E. B. Davis. | Hardin P. Cochrane. |
| 1893. | *Bedford Brown. | Joseph Price.<br>George A. Baxter. | *W. E. B. Davis. | Hardin P. Cochrane. |
| 1894. | *Cornelius Kollock. | A. B. Miles.<br>J. B. S. Holmes. | *W. E. B. Davis. | Hardin P. Cochrane. |
| 1895. | Louis McLane Tiffany. | Ernest S. Lewis.<br>Manning Simons. | *W. E. B. Davis. | Richard Douglas. |
| 1896. | Ernest S. Lewis. | Joseph Taber Johnson.<br>Richard Douglas. | *W. E. B. Davis. | A. M. Cartledge. |
| 1897. | George Ben Johnston. | W. E. Parker.<br>Floyd W. McRae. | *W. E. B. Davis. | A. M. Cartledge. |
| 1898. | Richard Douglas. | *H. H. Mudd.<br>James A. Goggans. | *W. E. B. Davis. | A. M. Cartledge. |
| 1899. | Joseph Taber Johnson. | F. W. Parham.<br>W. L. Robinson. | *W. E. B. Davis. | A. M. Cartledge. |
| 1900. | A. M. Cartledge. | Manning Simons.<br>W. P. Nicolson. | *W. E. B. Davis. | W. D. Haggard. |
| 1901. | Manning Simons. | George H. Noble.<br>L. C. Bosher. | W. D. Haggard. | Floyd W. McRae. |
| 1902. | *W. E. B. Davis. | J. Wesley Bovée.<br>J. W. Long. | W. D. Haggard. | Floyd W. McRae. |
| 1903. | J. Wesley Bovée. | Christopher Tompkins.<br>Bacon Saunders. | W. D. Haggard. | Floyd W. McRae. |
| 1904. | Floyd W. McRae. | George S. Brown.<br>J. Shelton Horsley. | W. D. Haggard. | Charles M. Rosser. |
| 1905. | Lewis C. Bosher. | J. D. S. Davis.<br>I. S. Stone. | W. D. Haggard. | Charles M. Rosser. |
| 1906. | George H. Noble. | Stuart McGuire.<br>E. Denegre Martin. | W. D. Haggard. | Charles M. Rosser. |
| 1907. | Howard A. Kelly. | Rufus E. Fort.<br>Hubert A. Royster. | W. D. Haggard. | Charles M. Rosser. |

* Deceased.

## HONORARY MEMBERS.

| | |
|---|---|
| 1894.—Foy, George, | Dublin. |
| 1894.—Jacobs, Charles, | Brussels. |
| 1894.—Martin, A., | Berlin. |
| 1902.—Maury, Richard B., | Memphis, Tenn. |
| 1894.—Morisani, O., | Naples. |
| 1894.—Pozzi, S., | Paris. |
| 1902.—Reamy, Thaddeus A., | Cincinnati, Ohio. |

# MEMBERS.

* Deceased.

1905.—Baker, James Norment, M.D. Visiting Surgeon, St. Margaret's Hospital; Secretary of the Medical Association of the State of Alabama. 719 Madison Avenue, Montgomery, Ala.

1894.—Baldridge, Felix E. Huntsville, Ala.

1900.—Baldy, John Montgomery, M.D. Professor of Gynecology, Philadelphia Polyclinic; Surgeon to Gynecean Hospital; Consulting Surgeon to Frederick Douglass Free Hospital and to the Jewish Hospital. 2219 De Lancey Place, Philadelphia, Pa.

1903.—Balloch, Edward A., M.D. Professor of Surgery, Medical Department of Howard University; Surgeon to Freedmen's Hospital; Member American Academy of Medicine. 1013 Fifteenth Street, N. W., Washington, D. C.

1905.—Barrow, David, M.D. Ex-President Kentucky State Medical Society. 148 Market Street, Lexington, Ky.

1905.—Bartlett, Willard, M.D. 4257 Washington Boulevard, St. Louis, Mo.

1904.—Batchelor, James M., M.D. House Surgeon of Charity Hospital; Associate Professor of Surgery, Tulane University. New Orleans, La.

1901.—Baughman, Greer D., M.D. Lecturer on Hygiene; Demonstrator of Pathology; Demonstrator of Physiology; in charge of Gynecological Dispensary at Medical College of Virginia. 12 North Second Street, Richmond, Va.

1907.—Bloodgood, Joseph Colt, M.D. Associate Professor of Surgery, Johns Hopkins University; Associate Surgeon, Johns Hopkins Hospital; Surgeon to the Union Protestant Infirmary; Chief Surgeon to St. Agnes Hospital. 904 North Charles Street, Baltimore, Md.

1893.—Bloom, J. D., M.D. Consulting Surgeon, Touro Infirmary; Lecturer on Diseases of Children, Tulane University. New Orleans, La.

1901.—Boldt, Hermann J., M.D. Gynecologist to Post-Graduate Hospital, St. Mark's Hospital, German Polyclinic; Consulting Gynecologist, Beth Israel Hospital; Professor of Gynecology, Post-Graduate Medical School. 39 East Sixty-first Street, New York, N. Y.

1902.—Bonifield, Charles Lybrand, M.D. Professor of Clinical Gynecology, Medical College of Ohio (Medical Department University of Cincinnati); Member American Association of Obstetricians and Gynecologists; ex-President Cincinnati Academy of Medicine; ex-President Cincinnati Obstetrical Society. 432 West Fourth Street, Cincinnati, O.

1890.—Bosher, Lewis Crenshaw, M.D. Professor of Practice of Surgery and Clinical Surgery in the Medical College of Virginia; Visiting Surgeon to the Memorial Hospital; Member of American Surgical Association; Member of the American Association of Obstetricians and Gynecologists; Member of Association of American Anatomists; Member of American Urological Association; ex-President of Richmond Academy of Medicine and Surgery. *Vice-President*, 1901; *President*, 1905. 422 East Franklin Street, Richmond, Va.

1895.—Bovée, J. Wesley, M.D. Fellow of American Gynecological Society; President of Washington Obstetrical and Gynecological Society; ex-President Medical and Surgical Society of the District of Columbia; Honorary Fellow Medical Society of Virginia; Professor of Gynecology of George Washington University, Washington, D. C.; Gynecologist to Providence, Columbia, and George Washington University Hospitals; Consulting Physician to St. Ann's Infant Asylum, and Attending Gynecologist to St. Elizabeth's Hospital for the Insane, Washington. D. C. *Vice-President*, 1902; *President*, 1903; *Council*, 1903-1906. The Rochambeau, Washington, D. C.

*1889.—Brokaw, Augustus V. L., M.D.

1902.—Broome, G. Wiley, M.D. Consulting Surgeon St. Louis City and Female Hospitals; Member St. Louis Medical Society. 612 N. Taylor Avenue, St. Louis, Mo.

1898.—Brown, George S., M.D. Ex-President Jefferson County Medical Society. *Vice-President*, 1904. Birmingham, Ala.

1905.—Brown, John Young, M.D. Formerly Surgeon-in-Charge, St. Louis City Hospital; President of the American Association of Obstetricians and Gynecologists, 1906. St. Louis, Mo.

1905.—Bullitt, James B., M.D. Secretary Kentucky State Medical Society. 205 West Broadway, Louisville, Ky.

1907.—Burch, Lucius E., M.D. Professor of Gynecology, Vanderbilt University, Medical Department; Gynecologist, St. Thomas' Hospital, Nashville; Gynecologist, Nashville City Hospital; Captain and Surgeon, N. G. S. T. 150 Eighth Avenue, N., Nashville, Tenn.

1899.—Burthe, L., M.D. 124 Baronne Street, New Orleans, La.

1900.—Byford, Henry T., M.D. Professor of Gynecology, College of Physicians and Surgeons (University of Illinois), Chicago; Professor of Clinical Gynecology, Northwestern University Woman's Medical School; Professor of Gynecology, Chicago Post-Graduate Medical School; Professor of Gynecology, Chicago Clinical School; Surgeon to Woman's Hospital; Gynecologist to Chicago West Side Hospital. Reliance Building, 100 State Street, Chicago, Ill.

1902.—Caldwell. C. E., M.D. Professor of Orthopedic Surgery, Miami Medical College; Visiting Orthopedic Surgeon, Cincinnati Hospital; Member Cincinnati Academy of Medicine. 1110 Cross Lane, Cincinnati, O.

1907.—Cannady, John Egerton, M.D. Superintendent and Surgeon-in-Charge, Sheltering Arms Hospital, Paint Creek, W. Va. Hansford, W. Va.

1904.—Carothers, Robert, M.D. Professor of Clinical Surgery, Medical College of Ohio, Medical Department, Cincinnati University; Surgeon to the Good Samaritan Hospital; 413 Broadway, Cincinnati, O.

1903.—Carr, William P., M.D. Professor of Physiology and Professor of Clinical Surgery, Columbian University; Surgeon to the Emergency Hospital and University Hospital; Consulting Surgeon to the Washington Asylum Hospital and the Government Hospital for the Insane, Washington, D. C. 1418 L Street, N. W., Washington, D. C.

1891.—Cartledge, Abiah Morgan, M.D. Professor of Gynecology and Abdominal Surgery, Louisville Medical

College; Visiting Surgeon to the Louisville City Hospital; Surgeon to the Louisville Home of the Friendless; ex-President of the Louisville Medico-Chirurgical Society. *Treasurer*, 1896–1899; *President*, 1900. 205 West Broadway, Louisville, Ky.

1899.—Cathcart, R. S., M.D. Assistant to Chair of Clinical Surgery in the Medical College of the State of South Carolina. 66 Hasell Street, Charleston, S. C.

1904.—Chambers, P. F., M.D. Attending Surgeon, Woman's Hospital; Consulting Surgeon, French Hospital. 49 West Fifty-seventh Street, New York, N. Y.

1907.—Clark, John G., M.D. Professor of Gynecology, University of Pennsylvania; Gynecologist-in-Chief, University Hospital. 218 South Fifteenth Street, Philadelphia, Penna.

1893.—Cocram, Henry S., M.D. 124 Baronne Street, New Orleans, La.

1893.—Coley, Andrew Jackson, M.D. Councillor of the Medical Association of the State of Alabama. Alexander City, Ala.

1902.—Coley, William B., M.D. Clinical Lecturer on Surgery, College of Physicians and Surgeons; Assistant Surgeon in Hospital for Ruptured and Crippled; Attending Surgeon to General Memorial Hospital; Fellow of the American Surgical Association, New York Surgical Society, and New York Academy of Medicine. 5 Park Avenue, New York, N. Y.

1904.—Craig, Daniel H., M.D. Surgeon to Out-patients, Free Hospital for Women; Instructor, Boston Polyclinic. 386 Commonwealth Avenue, Boston, Mass.

1902.—Crile, George W., M.D. Professor of Clinical Surgery, Medical College of Western Reserve University; Surgeon to St. Alexis' Hospital; Surgeon to Lakeside Hospital. 275 Prospect Street, Cleveland, O.

1893.—Crofford, T. J., M.D. Professor of Physiology and Clinical Lecturer on Gynecology in the Memphis Hospital Medical College. 155 Third Street, Memphis, Tenn.

1907.—Cullen, Thomas Stephen, M.D. Associate Professor of Gynecology, Johns Hopkins University; Associate in Gynecology, Johns Hopkins Hospital; Consultant in Abdominal Surgery, Church Home and Infirmary. Fellow of the

American Gynecological Society; Honorary Member to La Societa Italiana Ostettricia Ginecologia, Rome; Corresponding Member of the Gesellschaft f. Geburtshülfe, Leipzig. 3 West Preston Street, Baltimore, Md.

*Founder.*—Cunningham, Russell M., M.D. Professor of Practice of Medicine and Clinical Medicine, Birmingham Medical College; Surgeon to the Pratt Mines Hospital. Ensley, Ala.

1907.—Cushing, Harvey, M.D. Associate Professor of Surgery in the Johns Hopkins University. 3 West Franklin Street, Baltimore, Md.

1894.—Dandridge, Nathaniel Pendleton, M.D. Professor of Surgery, Miami Medical College; Surgeon to the Cincinnati Hospital and Episcopal Hospital for Children; ex-President of the American Surgical Society. 422 Broadway, Cincinnati, O.

*Founder.*—Davis, John D. S., M.D. Professor of Surgery, Birmingham Medical College; Surgeon to Hillman Hospital; ex-President of the Jefferson County Medical Society and of the Board of Health of Jefferson County. *Vice-President,* 1905. Avenue G and Twenty-first Street, Birmingham, Alabama.

1891.—Dean, G. R., M.D. Fellow of the American Association of Obstetricians and Gynecologists; ex-President of South Carolina State Medical Association; ex-President Association of Surgeons of Southern Railroad. Spartanburg, S. C.

1900.—Deaver, John B., M.D. Surgeon-in-Chief to the German Hospital. 1634 Walnut Street, Philadelphia.

1892.—Dixon, Archibald, M.D. Ex-President of the Kentucky State Medical Society; ex-President of the McDowell Medical Society; ex-President of the Mississippi Valley Medical Association. Henderson, Ky.

1897.—Dorsett, Walter B., M.D. Professor of Obstetrics and Clinical Gynecology, St. Louis University; Gynecologist to the Missouri Baptist Sanitarium and to Evangelical Deaconess' Hospital. 3941 North Belle Place, St. Louis, Mo.

1896.—Doughty, W. H., M.D. Professor of Special Surgery and Surgical Pathology, Medical Department, University of Georgia; Chief Surgeon to Charleston and Western Carolina Railroad. 822 Greene Street, Augusta, Ga.

*Founder.*—Douglas, Richard, M.D. Formerly Professor of Gynecology and Abdominal Surgery, Vanderbilt University; Fellow of the American Association of Obstetricians and Gynecologists, and of the British Gynecological Society. *Treasurer,* 1895; *Vice-President,* 1896; *President,* 1898. 110 Eighth Avenue, S., Nashville, Tenn.

1903.—Downes, Andrew J., M.D. 1725 Girard Avenue, Philadelphia, Pa.

1905.—Dudley, Emelius C., M.D. Professor of Gynecology, Northwestern University Medical School; Surgeon to St. Luke's and Wesley Hospital; Fellow of the American Gynecological Society; Corresponding Member of the Société Obstétricale et Gynecologique de Paris; Fellow of the British Gynecological Society; One of the Founders of the Congrès Périodique, International de Gynécologie et d'Obstetrique; ex-President of the Chicago Gynecological Society. 1617 Indiana Avenue, Chicago, Ill.

1890.—Earnest, John G., M.D. Professor of Clinical Gynecology, College of Physicians and Surgeons; formerly Professor of Gynecology, Southern Medical College; Visiting Gynecologist to the Grady Hospital. 44 Houston St., Atlanta, Ga.

1891.—Elkin, William Simpson, M.D. Professor of Operative and Clinical Surgery, Atlanta College of Physicians and Surgeons; ex-President of the Atlanta Society of Medicine; Surgeon to Grady Hospital. 27 Luckie Street, Atlanta, Ga.

1900.—Ellis, James N., M.D. Surgeon to Grady Hospital; Gynecologist to the Presbyterian Hospital. 1204 Fourth National Bank Building, Atlanta, Ga.

1900.—Ferguson, Alexander Hugh, M.D. Professor of Surgery, Chicago Post-Graduate; Professor of Surgery, Medical Department (College of Physicians and Surgeons), University of Illinois. 10 Drexel Square, Chicago, Ill.

1904.—Finney, J. M. T., M.D. Associate Professor of Surgery, Johns Hopkins University; Surgeon to Out-patients, Johns Hopkins Hospital. 1300 Eutaw Place, Baltimore, Md.

1901.—Fort, Rufus E., M.D. Chief Surgeon, Nashville Terminal Company; Surgeon to Southern and Illinois Central Railroads; formerly Superintendent Nashville Hospital. 209 Seventh Avenue, N., Nashville, Tenn.

1895.—Fortier, S. M., M.D. Visiting Surgeon to Charity Hospital. 4422 St. Charles Avenue, New Orleans, La.

1905.—Frank, Louis, M.D. Professor of Abdominal and Pelvic Surgery, Kentucky University Medical Department; Surgeon Louisville City Hospital. 110 West Kentucky Street, Louisville, Ky.

1902.—Freiberg, Albert H., M.D. Professor of Orthopedic Surgery, University of Cincinnati (Medical College of Ohio), and in the Laura Memorial Women's Medical College; Orthopedic Surgeon to the Cincinnati, Presbyterian, and Jewish Hospitals; Fellow of the American Orthopedic Association. 19 West Seventh Street, Cincinnati, O.

1895.—Fry, Henry D., M.D. Professor of Obstetrics, Georgetown University. 1601 Connecticut Avenue, N. W., Washington, D. C.

1903.—Gale, Joseph A., M.D. Surgeon-General Norfolk and Western R. R.; ex-President of Medical Society of Virginia. Roanoke, Va.

1900.—Gaston, J. McF., Jr., A.M., M.D. 421 Capitol Avenue, Atlanta, Ga.

1900.—Gerster, Arpad G., M.D. Attending Surgeon to Mt. Sinai Hospital; Consulting Surgeon to the German Hospital. 34 East Seventy-fifth Street, New York, N. Y.

1902.—Gillespie, Reese B., M.D. Surgeon to Tazewell Sanitarium. Tazewell, Va.

1897.—Glasgow, Frank A., M.D. Professor of Clinical Gynecology of St. Louis Medical College; Gynecologist to St. Louis Mullanphy Hospital; on staff of Martha Parsons Free Hospital; Consulting Surgeon to St. Louis Female Hospital. 3894 Washington Boulevard, St. Louis, Mo.

1903.—Goffe, J. Riddle, M.D. Professor of Gynecology, New York Polyclinic Medical School and Hospital; Gynecologist, New York City Hospital; Assistant Gynecologist to New York Skin and Cancer Hospital; Consulting Gynecologist, St. Joseph's Hospital, Yonkers, N. Y.; Fellow American Gynecological Society. 29 West Forty-sixth Street, New York, N. Y.

*Founder.*—Goggans, James A., M.D. Ex-President of the Tri-State Medical Society of Alabama, Georgia, and Tennessee; Senior Councillor of the Medical Association of the State of

Alabama; Fellow of the British Gynecological Society. *Vice-President*, 1898. Alexander City, Ala.

1900.—Goldsmith, W. S., M.D. 262 Jackson Street, Atlanta, Ga.

*Founder.*—Graham, Joseph, M.D. Member Charlotte, North Carolina, Medical Society; ex-Member North Carolina State Board of Health, and North Carolina State Board of Medical Examiners; ex-President North Carolina State Medical Society. Charlotte, N. C.

1892.—Grant, Horace H., M.D. Professor of Surgery in the Hospital College of Medicine; Surgeon to the Louisville City Hospital. 321 Equitable Building, Louisville, Ky.

1905.—Guerry, Le Grand, M.D. Surgeon Columbia Hospital. Columbia, S. C.

1896.—Gwathmey, Lomax, M.D. Attending Gynecologist to St. Vincent Hospital and Dispensary. Norfolk, Va.

1896.—Haggard, William David, M.D. Professor of Gynecology, Medical Department, University of Tennessee; formerly Professor of Gynecology and Abdominal Surgery, University of the South (Sewanee); Surgeon to St. Thomas' Hospital; Gynecologist to Nashville City Hospital; Member Alumni Association of Woman's Hospital, of New York. *Treasurer*, 1900; *Secretary*. 148 Eighth Avenue, N., Nashville, Tenn.

1892.—Hall, Rufus Bartlett, A.M., M.D. Professor of Clinical Gynecology in the Miami Medical College; Gynecologist to the Presbyterian Hospital; ex-President of the Cincinnati Obstetrical Society; ex-President of the American Association of Obstetricians and Gynecologists; Fellow of British Gynecological Society. 628 Elm Street, Cincinnati, O.

1901.—Hazen, Charles M., M.D. Professor of Physiology, Medical College of Virginia. Bon Air (Richmond), Va.

1896.—Heflin, Wyatt, M.D. Ex-President of the Jefferson County Medical Society. Birmingham, Ala.

1905.—Hendon, George A., M.D. Professor of Principles and Practice of Surgery and Surgical Pathology, Hospital College of Medicine. 1826 Baxter Avenue, Louisville, Ky.

*Founder.*—Herff, Ferdinand, M.D. 308 East Houston Street, San Antonio, Texas.

1889.—Hill, Robert Somerville, M.D. Councillor of

the Alabama State Medical Association; formerly Surgeon of the Second Regiment Alabama State Troops. Montgomery, Ala.

1902.—Hirst, Barton Cooke, M.D. Professor of Obstetrics, University of Pennsylvania; Fellow of College of Physicians, Philadelphia; Gynecologist to the Howard, Orthopedic, and Philadelphia Hospitals; Fellow of American Gynecological Society. 1821 Spruce Street, Philadelphia, Pa.

*Founder.*—Hogan, S. M., M.D. Grand Senior Councillor and ex-Vice-President of the Medical Association of the State of Alabama. Montgomery, Ala.

1900.—Hoke, Michael, M.D., B.E. 268 Peachtree Street, Atlanta, Ga.

1907.—Holden, Gerry R., M.D. Formerly House Surgeon, Roosevelt Hospital, New York City; formerly Resident Gynecologist, Johns Hopkins Hospital, and Assistant in Gynecology, Johns Hopkins Medical School. 223 West Forsyth Street, Jacksonville, Fla.

1891.—Holmes, J. B. S., M.D. Ex-President of the Tri-State Medical Society of Alabama, Georgia, and Tennessee; ex-President of the Georgia Medical Association; Fellow of the American Association of Obstetricians and Gynecologists. *Vice-President,* 1894. Atlanta, Ga.

1900.—Horsley, John Shelton, M.D. Professor of Principles of Surgery and Clinical Surgery, Medical College of Virginia; Surgeon to Memorial Hospital. *Vice-President,* 1904. 303 W. Grace Street, Richmond, Va.

1896.—Hundley, J. Mason, M.D. Associate Professor of Diseases of Women and Children, University of Maryland; President Clinical Society of Baltimore. 1009 Cathedral Street, Baltimore, Md.

1903.—Hunner, Guy Le Roy, M.D. Associate in Gynecology, Johns Hopkins University. 1420 Eutaw Place, Baltimore, Md.

1900.—Ill, Edward Joseph, M.D. Surgeon to the Woman's Hospital; Medical Director, St. Michael's Hospital; Gynecologist and Supervising Obstetrician, St. Barnabas' Hospital; Consulting Gynecologist to German Hospital and Beth Israel Hospital of Newark, N. J.; to All Souls' Hospital, Morristown, N. J., and to Mountain Side Hospital, Montclair, N. J.; Vice-

President from New Jersey to Pan-American Congress of 1893. 1002 Broad Street, Newark, N. J.

1889.—Johnson, Joseph Taber, M.D. Professor of Gynecology and Abdominal Surgery, Georgetown University; formerly Gynecologist to Providence Hospital; Chief of Service of Gynecology and Abdominal Surgery, Georgetown University Hospital, and President of the Board of Administration; President of the Woman's Dispensary, and of the Medical Society of the District of Columbia (1887); Member of the British Medical Association; Fellow of the Gynecological Society; President of the Washington Obstetrical and Gynecological Society; Secretary American Gynecological Society, 1886–1890, and President, 1899. *President*, 1899. 926 Farragut Square, Washington, D. C.

1901.—Johnson, Robert W., M.D. Professor of Surgery, Baltimore Medical College. Corner Franklin and Cathedral Streets, Baltimore, Md.

*Founder.*—Johnston, George Ben, M.D. Formerly Consulting Surgeon to the Richmond Eye, Ear, and Throat Infirmary; ex-President of the Richmond Academy of Medicine and Surgery; ex-President of the Medical Society of Virginia; Member of the American Association of Obstetricians and Gynecologists, and First Vice-President, 1896; Member of the Ninth International Medical Congress; Member of the American Surgical Association; late Professor of Anatomy in the Medical College of Virginia, and now Professor of the Practice of Surgery and Clinical Surgery; Surgeon to the Old Dominion Hospital; Consulting Surgeon to the City Free Dispensary. *Vice-President*, 1892; *President*, 1897. 407 East Grace Street, Richmond, Va.

1902.—Jordan, William Mudd, M.D. Surgeon to St. Vincent's Hospital; Gynecologist to Hillman Hospital; formerly Assistant Surgeon, U. S. Marine Hospital Service. Birmingham, Ala.

1890.—Kelly, Howard Atwood, M.D. Founder of Kensington Hospital, Philadelphia; Associate Professor of Obstetrics, University of Pennsylvania, 1888–89; Professor of Gynecology and Obstetrics, Johns Hopkins University, 1888–99; Professor of Gynecology, Johns Hopkins University; Gynecologist-in-Chief to Johns Hopkins Hospital; Membre Associé

Etrander de la Société Obstétricale et Gynecologique de Paris; Correspondirendes Mitglied der Gesellschaft f. Geburtshülfe zu Leipzig; Honorary Fellow Edinburgh Obstetrical Society, Royal Academy of Medicine (Ireland), and British Gynecological Society; Member Washington Academy of Sciences; Fellow American Gynecological Society. *President.* 1418 Eutaw Place, Baltimore, Md.

1899.—Kohlman, William, M.D. 3500 Prytania Street, New Orleans, La.

1894.—Leigh, Southgate, M.D., Visiting Surgeon to St. Vincent's and Norfolk Protestant Hospitals; First Vice-President Medical Society of Virginia, 1899–1900. 147 Granby St., Norfolk.

1893.—Lewis, Ernest S., M.D. Professor of Gynecology, Medical Department of Tulane University. *Vice-President,* 1895; *President,* 1896; *Member of Council,* 1896–1903. 124 Baronne Street, New Orleans, La.

1900.—Lloyd, Samuel, M.D. Professor of Surgery, New York Post-Graduate Medical School; Attending Surgeon, Post-Graduate Hospital, and to the Babies' Wards. 24 West Fiftieth Street, New York, N. Y.

*Founder.*—Long, John W., M.D. Emeritus Professor of Gynecology and Pediatrics in the Medical College of Virginia; formerly Gynecologist to the Old Dominion Hospital and the Richmond City Dispensary; Consulting Surgeon to the Watts Hospital, Durham; Member, ex-Orator, and ex-Vice-President of the North Carolina Medical Society. *Vice-President,* 1902. Greensboro, N. C.

1904.—Lupton, Frank A., M.D. 716 North Eighteenth Street, Birmingham, Ala.

1902.—McAdory, Wellington P., M.D. Surgeon to Hillman Hospital. Birmingham, Ala.

1895.—McGannon, M. C., M.D. Professor of Gynecology, Medical Department, University of Nashville; Chief Surgeon Woman's Hospital. 118 Eighth Avenue, N., Nashville, Tenn.

1893.—McGuire, Stuart, M.D. Professor of the Principles of Surgery and Clinical Surgery, University College of Medicine; Surgeon-in-Charge, St. Luke's Hospital; Visiting Surgeon, Virginia Hospital; Consulting Surgeon, Virginia Home for Incurables. 518 East Grace Street, Richmond, Va.

1891.—McGuire, W. Edward, M.D. Professor of Gyne-

cology, University College of Medicine; Gynecologist to the Virginia Hospital. 411 East Grace Street, Richmond, Va.

1905.—McLean, Henry Stuart, M.D. Professor of Pathology, University College of Medicine. 406 West Grace Street, Richmond, Va.

1888.—McMurtry, Lewis S., M.D. Ex-President of the American Medical Association; Professor of Gynecology and Abdominal Surgery, Hospital College of Medicine, Louisville; Gynecologist to St. Mary and St. Elizabeth Hospital; Gynecologist to Gray Street Infirmary; ex-President of the American Association of Obstetricians and Gynecologists; ex-President of the Kentucky State Medical Society; Corresponding Member of the Gynecological Society of Boston; Fellow of the British Gynecological Society; Ordinary Fellow of the Edinburgh Obstetrical Society; Fellow of the American Surgical Society. *President,* 1891; *Member of Council,* 1892–1904. Masonic Temple, Louisville, Ky.

1888.—McRae, Floyd Willcox, M.D. Professor of Gastro-intestinal, Rectal, and Clinical Surgery, Atlanta College of Physicians and Surgeons; Surgeon to Grady Hospital; ex-President Medical Association of Georgia; ex-Secretary of Section on Surgery and Anatomy, American Medical Association. *Treasurer,* 1901–1903; *President,* 1904. Peters Building, Atlanta, Ga.

1900.—MacDonald, Willis G., M.D. Professor of Surgery (Adjunct), Albany Medical College; Surgeon to Albany Hospital; Chief Surgeon of South End Dispensary; Fellow of the American Surgical Society. 29 Eagle Street, Albany, N. Y.

1900.—Manton, Walter Porter, M.D. Professor of Clinical Gynecology and Adjunct Professor of Obstetrics, Detroit College of Medicine; Gynecologist to Harper Hospital, Eastern and Northern Michigan Asylums for the Insane; Consulting Gynecologist to St. Joseph's Retreat; Vice-President of Medical Board of the Woman's Hospital and Infants' Home. 32 Adams Avenue, Detroit, Mich.

1891.—Marcy, Henry Orlando, M.D., LL.D. Ex-President of the American Medical Association; President of the Section of Gynecology, Ninth International Medical Congress; late President of the American Academy of Medicine; Corresponding Member of the Medico-Chirurgical Society of Bologna,

Italy; Fellow of the American Association of Obstetricians and Gynecologists. 180 Commonwealth Avenue, Boston, Mass.

1899.—Martin, Edmund Denegre, M.D. Professor of Minor and Clinical Surgery, New Orleans Polyclinic; Visiting Surgeon to the Charity Hospital; Consulting Surgeon to the Eye, Ear, Nose, and Throat Hospital; President Orleans Parish Medical Society; Ex-Vice-President American Medical Association. 115 Chartres Street, New Orleans, La.

1904.—Mason, J. M., M.D. Alternate Surgeon to St. Vincent Hospital. 2010 Thirteenth Avenue, Birmingham, Ala.

1899.—Mastin, William M., M.D. Fellow of the American Surgical Association; Member of the American Association of Genito-urinary Surgeons; Surgeon to the Mobile City Hospital. Joachim and Conti Streets, Mobile, Ala.

1893.—Matas, R., M.D. Professor of Surgery, Medical Department of Tulane University; Fellow of the American Surgical Association. 624 Gravier Street, New Orleans, La.

1896.—Matthews, William P., M.D. Professor of Anatomy, Medical College of Virginia; Orthopedic Surgeon to the Old Dominion Hospital. Richmond, Va.

1902.—Mayo, Charles H., M.D. Attending Surgeon, St. Mary's Hospital; Member American Surgical Society; ex-President, Western Surgical and Gynecological Association and of Minnesota State Medical Society. Rochester, Minn.

*Founder.*—Meriwether, Frank T., M.D. Assistant Surgeon, U. S. A. (Retired). Asheville, N. C.

1899.—Michinard, P. E., M.D. 624 Gravier Street, New Orleans, La.

1888.—Miller, C. Jeff., M.D. Professor of Operative Gynecology, New Orleans Polyclinic; Visiting Gynecologist to Charity Hospital. 1719 Jackson Avenue, New Orleans, La.

1899.—Milliken, Samuel E., M.D. 449 Elm Street, Dallas, Texas.

1901.—Mixter, Samuel Jason, M.D. Surgeon, Massachusetts General Hospital; Consulting Surgeon, Massachusetts Charity Eye and Ear Infirmary; Instructor of Surgery, Harvard Medical School. 180 Marlborough Street, Boston, Mass.

1904.—Moran, John F., M.D. Professor of Obstetrics, Georgetown University; Obstetrician to Georgetown University

Hospital and Columbia Hospital for Women. Washington, D. C.

1903.—Morgan, James B., M.D. Professor of Anatomy and Clinical Surgery, Medical College of Georgia; Chairman Board of Censors, Georgia Medical Association. 1272 Broad Street, Augusta, Ga.

1902.—Morris, Lewis C., M.D. Professor of Anatomy and Associate Professor of Gynecology, Birmingham Medical College; Councillor of Medical Association, State of Alabama; Vice-President of Tri-State Medical Society of Alabama, Georgia, and Tennessee. 716 North Eighteenth Street, Birmingham, Ala.

1900.—Morris, Robert Tuttle, M.D. Professor of Surgery, New York Post-Graduate Medical School and Hospital. 616 Madison Avenue, New York, N. Y.

1901.—Mullally, Lane, M.D. Professor of Obstetrics and Diseases of Children, Medical College State of South Carolina; Visiting Physician in Obstetrics and Pediatrics, Roper Hospital. Charleston, S. C.

1905.—Munro, John Cummings, M.D. Surgeon-in-Chief, Carney Hospital; Consulting Surgeon, Quincy Hospital and Framingham Hospital; President of the Society of Clinical Surgery; Member American Surgical, American Academy, and of the Association of American Anatomists. 173 Beacon Street, Boston, Mass.

1891.—Murfree, James Brickle, M.D. Professor of Surgery, University of the South (Sewanee); ex-President of the Tennessee State Medical Society, of the Tri-State Medical Society of Alabama, Georgia, and Tennessee. Murfreesboro, Tenn.

1894.—Murphy, J. B., M.D. Professor of Surgery, Rush Medical College, in affiliation with the University of Chicago; Professor of Surgery, Post-Graduate School and Hospital; Attending Surgeon to the Presbyterian, Cook County, and Mercy Hospitals. 100 State Street, Chicago, Ill.

*Founder.*—Nash, Herbert Milton, M.D. Consulting Surgeon to the Retreat for the Sick (Hospital), Norfolk, Consultant to the Staff of St. Vincent's Hospital; ex-President of the Norfolk City Medical Society; Member of the State Board of Medical Examiners of Virginia. 296 Freemason Street, Norfolk, Va.

1889.—Nicolson, William Perrin, M.D. Professor of Anatomy and Clinical Surgery, Atlanta College of Physicians and Surgeons. *Vice-President*, 1900. Prudential Building, Atlanta, Ga.

1896.—Noble, Charles P., M.D. Surgeon-in-Chief, Kensington Hospital for Women; Consulting Surgeon, Woman's Hospital; Lecturer on Gynecology, Philadelphia Polyclinic. 1500 Locust Street, Philadelphia, Pa.

1890.—Noble, George H., M.D. Professor of Abdominal Surgery and Clinical Gynecology in the Atlanta School of Medicine; Dean of same; Gynecologist of Grady Hospital; Senior Gynecologist, Wesleyan Memorial Hospital, Atlanta; Fellow of the American Gynecological Association and the American Association of Obstetricians and Gynecologists; ex-President of the Medical Association of Georgia; ex-Secretary of the Section of Obstetrics, American Medical Association. *Vice-President*, 1901; *President*, 1905–06. 131 South Pryor Street, Atlanta, Ga.

1903.—Nolen, William L., M.D. Visiting Surgeon, Erlanger Hospital; ex-President Chattanooga Medical Society. Loveman Building, Chattanooga, Tenn.

1890.—Nunn, R. J., M.D. Savannah, Ga.

1902.—O'Brien, Matthew Watson, M.D. Surgeon Southern, and Chesapeake and Ohio Railways; Member of the American Association for the Advancement of Science. 908 Cameron Street, Alexandria, Va.

1901.—Ochsner, A. J., M.D. Adjunct Professor of Clinical Surgery, College of Physicians and Surgeons; Surgeon-in-Chief Augustana and St. Mary's Hospital. 710 Sedgwick Street, Chicago, Ill.

1903.—Ochsner, Edward H., M.D. Surgeon to Augustana and St. Mary's Hospital. 710 Sedgwick Street, Chicago, Ill.

1904.—Oechsner, John F., M.D. Professor of Orthopedics, New Orleans Polyclinic; Assistant Demonstrator of Anatomy, Tulane University; Visiting Surgeon, Charity Hospital; 124 Baronne Street, New Orleans, La.

1902.—Oliver, John Chadwick, M.D. Dean and Professor of Operative Surgery, Miami Medical College; Surgeon to the Cincinnati, Christ, and Presbyterian Hospitals. 628 Elm Street, Cincinnati, Ohio.

1902.—Oviatt, Charles W., M.D. Formerly Professor of Surgery in Milwaukee Medical College; Attending Surgeon St. Mary's Hospital; Member American Surgical Society. Oshkosh, Wis.

*Founder.*—Paine, John Fannin Young, M.D. Professor of Obstetrics and Gynecology in the Medical Department, University of Texas; Gynecologist and Obstetrician, John Sealy Hospital; Fellow of the American Association of Obstetricians and Gynecologists; ex-President of the Texas Medical Association; Fellow of the Texas Academy of Science. 2177 Market Street, Galveston, Texas.

1888.—Parham, F. W., M.D. Professor of Surgery in the New Orleans Polyclinic. *Vice-President,* 1899. 1429 Seventh Street, New Orleans, La.

1900.—Park, Roswell, M.D. Professor of Surgery, Medical Department, University of Buffalo; Surgeon, Buffalo General Hospital; Member German Congress of Surgeons, Italian Surgical Society, American Surgical Association, American Orthopedic, and American Genitourinary Associations. 510 Delaware Avenue, Buffalo, N. Y.

1893.—Parker, William E., M.D. Formerly Visiting Surgeon to the Charity Hospital of New Orleans. *Vice-President,* 1897. Dugan-Stuart Building, Hot Springs, Ark.

1899.—Perkins, W. M., M.D. Chief of the Clinic Chair of General Clinical and Operative Surgery, New Orleans Polyclinic; Assistant Demonstrator of Operative Surgery, Medical Department, Tulane University of Louisiana; Visiting Surgeon to the Charity Hospital. 211 Camp Street, New Orleans, La.

1904.—Peters, J. U. W., M.D. Surgeon to St. Vincent Hospital. Birmingham, Ala.

1899.—Platt, Walter Brewster, M.D. Surgeon to the Robert Garrett Hospital for Children. 802 Cathedral Street, Baltimore, Md.

1889.—Polk, William M., M.D. Dean and Professor of Obstetrics and Gynecology, Cornell University Medical College; Gynecologist to Bellevue Hospital; Consulting Surgeon to St. Luke's, St. Vincent's, New York Lying-in, and Trinity Hospitals; ex-President of American Gynecological Society, and New York Obstetrical Society. 7 East Thirty-sixth Street, New York.

1900.—Porter, Miles F., M.D. Trustee of American Medical Association. 47 West Wayne Street., Fort Wayne, Ind.

1889.—Potter, William Warren, M.D. Consulting Gynecologist to the Woman's Hospital; Consulting Surgeon to the Buffalo General Hospital; President and Examiner in Obstetrics, New York State Medical Examining and Licensing Board; Chairman of Section of Obstetrics and Diseases of Women, American Medical Association, 1890; President of the Buffalo Obstetrical Society, 1884–1886; President of the Medical Society of the State of New York, 1891; Executive President of the Section of Gynecology and Abdominal Surgery, Pan-American Medical College, 1893. 284 Franklin Street, Buffalo, N. Y.

1889.—Price, Joseph, M.D. Physician-in-Charge of the Obstetrical and Gynecological Department of the Philadelphia Dispensary; Fellow of the American Association of Obstetricians and Gynecologists; ex-Chairman, Section of Diseases of Women and Obstetrics, American Medical Association. *Vice-President*, 1893. 241 North Eighteenth Street, Philadelphia, Pa.

1899.—Quinn, W. A., M.D. Henderson, Ky.

1900.—Ransohoff, Joseph, M.D., F.R.C.S. Professor of Anatomy and Clinical Surgery, Medical College of Ohio; Surgeon to Cincinnati, Good Samaritan, and Jewish Hospitals; Member American Surgical Association. 19 West Seventh Street, Cincinnati, Ohio.

1890.—Reed, Charles Alfred Lee, M.D. Ex-President of the American Medical Association; Professor of Gynecology and Abdominal Surgery in the Cincinnati College of Medicine and Surgery; Surgeon to the Cincinnati Free Hospital for Women; Fellow of the American Association of Obstetricians and Gynecologists; Fellow of the British Gynecologcial Society; ex-Chairman of the Section on Obstetrics and Diseases of Women of the American Medical Association; Secretary-General of the Pan-American Medical Congress, 1893; Honorary Member of the Medical Society of the State of New York. Corner Seventh and Race Streets, Cincinnati, Ohio.

1900.—Rees, Charles Mayrant, M.D. Professor of Abdominal Surgery and Gynecology, Charleston Medical School; Abdominal Surgery and Gynecology, City Hospital; Gynecologist to Shirro's Dispensary. Charleston, S. C.

1899.—Richardson, Maurice Howe, M.D. Visiting Surgeon to the Massachusetts General Hospital; Assistant Professor of Clinical Surgery, Harvard Medical School. 224 Beacon Street, Boston, Mass.

1888.—Roberts, William O., M.D. Professor of Principles of Surgery and Clinical Surgery; Medical Department of the University of Louisville; formerly Secretary of the Surgical Section of the American Medical Association. *Vice-President,* 1889. 1520 Third Street, Louisville, Ky.

1901.—Robins, Charles Russell, M.D. Professor of Gynecology, Medical College of Virginia; Gynecologist to the Memorial Hospital; Surgeon to the Atlantic Coast Railroad. 8 West Grace Street, Richmond, Va.

1889.—Robinson, William Lovaille, M.D. Fellow of the American Association of Obstetricians and Gynecologists; ex-President of the Danville Medical Society; Member of the Medical Examining Board of Virginia; ex-President of Virginia Medical Society. *Vice-President,* 1899. 753 Main Street. Danville, Va.

1907.—Rogers, Carey Pegram, M.D. Surgeon to De Soto Sanatorium; Surgeon, Sea Board Air Line Railway; Formerly House Surgeon, Johns Hopkins Hospital. 223 West Forsyth Street, Jacksonville, Fla.

1901.—Rogers, Mack, M.D. Professor of Anatomy, Birmingham Medical College. 212 Twentieth Street, Birmingham, Ala.

1901.—Rosser, Charles M., M.D. Professor of Surgery, Baylor University College of Medicine; Consulting Surgeon, Parkland Hospital; Attending Surgeon, Texas Baptist Memorial Sanitarium; ex-Vice-President Texas State Medical Association; Vice-President Tri-State Society of Texas, Louisiana and Arkansas. *Treasurer.* 432 Gaston Avenue, Dallas, Texas.

1899.—Royster, Hubert A., A.B., M.D. Professor of Gynecology and Dean of the Faculty, Medical Department, University of North Carolina; Gynecologist to Rex Hospital; Surgeon-in-Chief, St. Agnes Hospital. Tucker Building, Raleigh, N. C.

1901.—Runyan, Joseph P., M.D. 203 West Second Street, Little Rock, Ark.

1893.—Saunders, Bacon, M.D. Professor of Surgery and Clinical Surgery and Dean of Faculty of Medical Department,

Fort Worth University; ex-President Texas State Medical Association. *Vice-President*, 1903. Ninth and Houston Streets, Fort Worth, Texas.

*Founder.*—SCHILLING, NICHOLAS, M.D. Cedar Bayou, Texas.

1899.—SHANDS, A. R., M.D. Professor of Orthopedic Surgery in the Medical Department of the George Washington University and in the University of Vermont; Orthopedic Surgeon to the George Washington University Hospital, the Emergency Hospital and Central Dispensary; Charter Member Washington Academy of Sciences; Member American Orthopedic Association; and Honorary Member of Medical Society of Virginia. 901 Sixteenth Street, N. W., Washington, D. C.

1902.—SHERRILL, J. GARLAND, M.D. Professor of Surgery and Clinical Surgery, Kentucky University; Consulting Surgeon to Louisville Hospital. 633 St. Charles Place, Louisville, Ky.

*Founder.*—SHIVERS, OFFA LUNDSFORD, M.D. Councillor of the Alabama Medical Association. Marion, Ala.

1893.—SIMONS, MANNING, M.D. Professor of Clinical Surgery, Medical College, State of South Carolina; Surgeon to St. Francis Xavier's Infirmary; Surgeon to City Hospital; ex-President Medical Society of South Carolina; President of the South Carolina Medical Association; Member American Association of Obstetricians and Gynecologists; ex-Vice-President Tri-State Medical Society of Virginia and the Carolinas. *Vice-President*, 1895 and 1900; *President*, 1901. 111 Church Street, Charleston, S. C.

1900.—SIMPSON, FRANK FARROW, M.D. Assistant Gynecologist to Mercy Hospital; Gynecologist to Out-patient Department, Mercy Hospital. 524 Pennsylvania Avenue, Pittsburgh, Pa.

1894.—SMITH, REGINALD K., M.D. Surgeon U. S. Navy. 2600 Jackson Street, San Francisco, Cal.

1899.—SMYTHE, FRANK DAVIE, M.D. Surgeon to St. Joseph's Hospital; Assistant Professor of Surgery, Memphis Hospital Medical College; Demonstrator of Operative Surgery; formerly Member of the State Board of Examiners of Mississippi. Porter Building, Memphis, Tenn.

1904.—STOKES, JAMES ERNEST, M.D. Salisbury, N. C.

*Founder.*—Stone, Isaac Scott, M.D. Clinical Professor Gynecology, Georgetown University; Gynecologist to Columbia Hospital, and Associate Gynecologist to Georgetown University Hospital; Member American and British Gynecological Societies; Honorary Fellow of Medical Society of the State of New York; Fellow of the Medical Society of Virginia; Member Washington Academy of Sciences; Washington Obstetrical and Gynecological Society. 1618 Rhode Island Avenue, N.W. Washington, D. C.

1894.—Talley, D. F., M.D. Ex-President of the Jefferson County Medical Society; Professor of Materia Medica and Lecturer on Surgery, Birmingham Medical College. Birmingham, Ala.

*Founder.*—Taylor, Hugh M., M.D. Professor of Practice of Surgery and Clinical Surgery, University College of Medicine; Surgeon to the Virginia Hospital; ex-President of the Virginia Board of Medical Examiners. 6 North Fifth Street, Richmond, Va.

1898.—Taylor, William Wood, A.B., M.D. Gynecologist to St. Joseph's and the Memphis City Hospitals. 246 Randolph Building, Memphis, Tenn.

1892.—Thompson, J. E., M.D. Professor of Surgery in the University of Texas. Galveston, Texas.

1891.—Tiffany, Louis McLane, M.D. Fellow of the American Surgical Association. *President,* 1895. 831 Park Avenue, Baltimore, Md.

1890.—Tompkins, Christopher, M.D. Professor of Obstetrics in the Medical College of Virginia. *Vice-President,* 1903. 116 East Franklin Street, Richmond, Va.

1888.—Tuholske, Herman, M.D. Professor of Surgery, Medical Department, Washington University; Consulting Surgeon, St. Louis City Hospital; Surgeon to St. Louis Polyclinic Hospital, etc. 410 North Jefferson Street, St. Louis, Mo.

1905.—Vance, Ap Morgan, M.D. Surgeon to St. Mary and Elizabeth Hospital; Consulting Surgeon, Louisville City Hospital. 921 Fourth Street, Louisville, Ky.

1893.—Vander Veer, Albert, M.D. Professor of Clinical, Didactic, and Abdominal Surgery, Albany Medical College; Fellow of the American Surgical Association; ex-President of the American Association of Obstetricians and Gynecologists;

ex-President of the Medical Society of the State of New York. 28 Eagle Street, Albany, N. Y.

1891.—Walker, Edwin, M.D., Ph.D. Gynecologist to the Evansville City Hospital; ex-President Mississippi Valley Medical Association; President of the Indiana State Medical Society; Fellow of the American Association of Obstetricians and Gynecologists. 712 Upper Fourth Street, Evansville, Ind.

1905.—Wathen, John R., M.D. Professor of Surgery, Kentucky School of Medicine; Surgeon to St. Anthony's Hospital; Surgeon to Louisville City Hospital and to Kentucky School of Medicine Hospital; President Louisville Clinical Society. 628 Fourth Avenue, Louisville, Ky.

1898.—Watkins, Isaac Lafayette, M.D. Ex-President of the Montgomery County Medical and Surgical Society. Montgomery, Ala.

1901.—Werder, X. O., M.D. Gynecologist, Mercy Hospital, Pittsburgh; Professor of Gynecology, West Pennsylvania Medical College; Consulting Surgeon to the South Side Hospital, Allegheny General Hospital, St. Francis' Hospital, etc. 524 Pennsylvania Avenue, Pittsburgh, Pa.

1888.—Westmoreland, Willis F., M.D. Professor of Surgery, Atlanta College of Physicians and Surgeons. 241 Equitable Building, Atlanta, Ga.

1904.—Whaley, T. P., M.D. Lecturer Genito-Urinary and Renal Surgery, Charleston Medical School; Lecturer Diseases of Skin, Medical College of the State of South Carolina; Surgeon to Sherra's Dispensary; Visiting Surgeon, Charleston City Hospital. 113 Wentworth Street, Charleston, S. C.

1902.—Whitacre, Horace J., M.D., B.S. Professor of Pathology and Surgeon to Clinics, Medical Department, University of Cincinnati; Surgeon to Christ Hospital; Consulting Surgeon to Speer's Hospital; President Cincinnati Research Society; Assistant Pathologist Cincinnati Hospital. 831 Union Trust Building, Cincinnati, O.

1900.—Williams, Howard J., A.B., A.M., M.D. Surgeon to Macon Hospital; ex-President of the Medical Association of Georgia, etc. 550 Orange Street, Macon, Ga.

1900.—Williams, J. Whitridge, A.B., M.D. Professor of Obstetrics, Johns Hopkins University; Obstetrician-in-Chief,

Johns Hopkins Hospital; Gynecologist to the Union Protestant Infirmary. 1128 Cathedral Street, Baltimore, Md.

*Founder.*—Wilson, J. T., M.D. Ex-President of the North Texas Medical Association; President of the Texas Medical Association; Superintendent of the State Hospital for the Insane. *Vice-President,* 1891. Sherman, Texas.

1905.—Winslow, Randolph, M.D. Professor of Surgery, University of Maryland; Chief Surgeon, University Hospital; Consulting Surgeon, Hebrew Hospital, and to the Hospital for Crippled Children. 1900 Mt. Royal Terrace, Baltimore, Md.

1905.—Witherspoon, T. Casey, M.D. Formerly Professor of Operative and Clinical Surgery, Medical Department, St. Louis University; Member American Association of Anatomists. 307 West Granite Street, Butte, Mont.

1891.—Wyeth, John Allen, M.D. Ex-President of the American Medical Association (1901). Professor of Surgery in the New York Polyclinic; Consulting Surgeon to St. Elizabeth's Hospital; Visiting Surgeon to Mt. Sinai Hospital and Polyclinic Hospital; Honorary member of the Texas State Medical Association; ex-President of the New York State Medical Association; ex-President of the New York Pathological Society. 19 West Thirty-fifth Street, New York, N. Y.

1900.—Wysor, John C., M.D. Surgeon-in-Charge, Chesapeake and Ohio Hospital. Clifton Forge, Va.

1900.—Young, Hugh H., M.D. Chief of Clinic, Genito-Urinary Surgery, Johns Hopkins Hospital. 1005 North Charles Street, Baltimore, Md.

1902.—Zinke, E. Gustav, M.D. Professor of Obstetrics in Medical College of Ohio; Obstetrician and Gynecologist German Protestant Hospital; Attending Obstetrician to Ohio Maternity; Fellow of American Association of Obstetricians and Gynecologists. 13 Garfield Place, Cincinnati, O.

# CONSTITUTION.

## ARTICLE I.

The name of this Association shall be THE SOUTHERN SURGICAL AND GYNECOLOGICAL ASSOCIATION.

## ARTICLE II.

The object of this Association is to further the study and practice of surgery and gynecology among the profession of the Southern States.

## ARTICLE III.

This Association shall adopt and conform to the Code of Ethics of the American Medical Association.

## ARTICLE IV.

SECTION 1. Any reputable physician who practises surgery or gynecology, and who is vouched for by two members of the Association and recommended by the Council, shall be eligible to membership in this body.

SEC. 2. The honorary members shall not exceed ten in number, and shall enjoy all the privileges of other members, excepting to vote or hold office, but shall not be required to pay any fee.

## ARTICLE V.

SECTION 1. The officers of this Association shall be a President, two Vice-Presidents, a Secretary, a Treasurer, and a Council, elected by ballot.

SEC. 2. The President and Vice-Presidents shall be elected for one year, and the President shall not be eligible for re-election at any time; the Secretary and Treasurer, each, for five years; and the Council as provided for in the By-laws.

## ARTICLE VI.

SECTION 1. It shall be the duty of the President to preside at all meetings of the Association; to give the casting vote; to see that the rules of order and decorum be properly enforced in all deliberations of the Association; to sign the approved proceedings of each meeting, and to approve such orders as may be drawn upon the Treasurer for expenditures ordered by the Association.

SEC. 2. In the absence of the President the first Vice-President shall preside, and in his absence the second Vice-President shall preside.

SEC. 3. In the absence of all three, the Association shall elect one of its members to preside *pro tem.*

SEC. 4. It shall be the duty of the Secretary to keep a true and correct record of the proceedings of the meetings; to preserve all books, papers, and articles belonging to the archives of the Association; to attest all orders drawn on the Treasurer for moneys appropriated by the Association; to keep the account of the Association with its members; to keep a register of the members, with the dates of their admission and places of residence. He shall collect all moneys due from the members and pay to the Treasurer, taking his receipt for the same. He shall report such unfinished business of previous meetings as may appear on his books requiring action, and attend to such other business as the Association may direct. He shall also supervise and conduct all the correspondence of the Association, and edit the TRANSACTIONS under the direction of the Council.

SEC. 5. It shall be the duty of the Treasurer to keep a correct record of all moneys received from the hands of the Secretary, giving his receipt for the same; pay them out by order of the Association as indorsed by the President and attested by the seal in the hands of the Secretary.

SEC. 6. It shall be the duty of the President of the Association to appoint an Auditing Committee, consisting of three members of the Association, whose duty it shall be to examine the books of the Secretary and Treasurer, and report on the same on the last day of the session.

## ARTICLE VII.

Vacancies occurring in the offices of the Association shall be filled by appointment of the President until the next meeting. He shall also have the appointment of all committees not otherwise provided for.

## ARTICLE VIII.

This Constitution shall take effect immediately from the time of its adoption, and shall not be amended except by a written resolution, which shall lie over one year, and receive a vote of two-thirds of the members present.

## ARTICLE IX.

Any member who for three consecutive years fails to attend the meetings or present a paper shall be dropped from the roll of membership.

## ARTICLE X.

The membership of the Association shall be limited to two hundred.

## ARTICLE XI.

All members who have been in continuous membership for twenty years and have attended fifty per cent. of the meetings shall become life members and be exempt from dues.

# BY-LAWS.

## ARTICLE I.

The Southern Surgical and Gynecological Association shall meet annually on the second Tuesday of November at 10 A.M., at such place as may be designated by the preceding meeting.

## ARTICLE II.

The members present shall constitute a quorum for business.

## ARTICLE III.

The annual dues of each member shall be $10, paid in advance

## ARTICLE IV.

The usual parliamentary rules governing deliberative bodies shall govern the business workings of this Association.

## ARTICLE V.

All questions before the Association shall be determined by a majority of the votes present.

## ARTICLE VI.

The President shall deliver an annual address at each meeting of the Association.

## ARTICLE VII.

The Secretary of the Association shall receive at each annual Session, a draft from the President, drawn on the Treasurer, for the sum of $500 for services rendered the Association, and to this shall be added the necessary expense incurred in the discharge of his official duties.

## ARTICLE VIII.

It shall be the duty of the Secretary, one month prior to the annual meeting, to notify the members of the Association, and urge their attendance.

## ARTICLE IX.

The authors of papers shall notify the Secretary, six weeks prior to the meeting, of the titles of their essays, so that they may be incorporated in the preliminary programme.

## ARTICLE X.

### COUNCIL.

The Council shall consist of five members; and of those elected at the primary meeting, the first shall serve five years, the second four, the third three, the fourth two, and the fifth one year; so that subsequently one member of the Council shall be elected annually to serve five years. The President and Secretary shall be *ex-officio* members of the Council.

This Council shall organize by electing a Chairman and Secretary, and shall keep a record of its proceedings.

The duties of this Council shall be—

1. To investigate applications for membership and report to the Association the names of such persons as are deemed worthy.

2. To take cognizance of all questions of an ethical, judicial, or personal nature, and upon these the decision of the Council shall be final; *provided*, that appeal may be taken from such decision of the Council to the Association, under a written protest, which protest shall be sustained by the Association, and the matter shall then be referred to a special committee, with power to take final action.

3. All motions and resolutions before the Association shall be referred to the Council without debate, and it shall report by recommendation at as early an hour as possible.

## ARTICLE XI.

The President shall appoint at each annual meeting a Committee of Arrangements.

## ARTICLE XII.

The Council shall have full power to omit from the published TRANSACTIONS, in part or in whole, any paper that may be referred to it by the Association, unless specially instructed to the contrary by the Association, which will be determined by vote.

## ARTICLE XIII.

Any member failing to pay his dues for more than one year shall be dropped.

## ARTICLE XIV.

No paper shall be read before this Association which does not deal strictly with a subject of surgical or gynecological importance.

## ARTICLE XV.

No paper read before the Association shall be published in any medical journal or pamphlet for circulation, as having been read before the Association, without having received the indorsement of the Council.

## ARTICLE XVI.

The reading of papers shall be limited to twenty minutes each, except by permission of the Association.

MINUTES OF THE PROCEEDINGS

AT THE

# NINETEENTH ANNUAL MEETING

OF

# THE SOUTHERN

# SURGICAL AND GYNECOLOGICAL

# ASSOCIATION,

HELD AT THE

HOTEL BELVEDERE,

*Baltimore, Maryland,*

DECEMBER 11, 12 AND 13, 1906.

## NINETEENTH ANNUAL MEETING.

FIRST DAY.—*Tuesday, December* 11, 1906.

The following-named members were present:

| | |
|---|---|
| BALDRIDGE, F. E. | Huntsville. |
| BALDY, JOHN MONTGOMERY | Philadelphia. |
| BALLOCH, EDWARD A. | Washington. |
| BLOODGOOD, JOS. C. | Baltimore. |
| BOLDT, HERMANN J. | New York. |
| BOSHER, LEWIS C. | Richmond. |
| BOVÉE, J. WESLEY | Washington. |
| BYFORD, HENRY J. | Chicago. |
| CALDWELL, C. E. | Cincinnati. |
| CAROTHERS, ROBERT | Cincinnati. |
| CARR, WILLIAM P. | Washington. |
| CARTLEDGE, A. M. | Louisville. |
| CHAMBERS, P. F. | New York. |
| CLARK, JOHN G. | Philadelphia. |
| COLEY, A. J. | Alexander City. |
| COLEY, WILLIAM B. | New York. |
| CRILE, GEO. W. | Cleveland. |
| CUSHING, HARVEY | Baltimore. |
| CUNNINGHAM, R. M. | Ensley. |
| DEAVER, JOHN B. | Philadelphia. |
| FINNEY, J. M. T. | Baltimore. |
| FORT, RUFUS E. | Nashville. |
| FRANK LOUIS | Louisville. |
| FRY, HENRY D. | Washington. |
| GOFFE, J. RIDDLE | New York. |
| GOLDSMITH, W. S. | Atlanta. |
| HAGGARD, W. D. | Nashville. |
| HALL, RUFUS B. | Cincinnati. |
| HILL, ROBERT S. | Montgomery. |
| HOKE, MICHAEL | Atlanta. |
| HUNDLEY, J. MASON | Baltimore. |

| | |
|---|---|
| Hunner, Guy Leroy | Baltimore. |
| Ill, Edward J. | Newark. |
| Johnson, Joseph Taber | Washington. |
| Johnson, Robert W. | Baltimore. |
| Johnston, George Ben | Richmond. |
| Jordan, William M. | Birmingham. |
| Kelly, Howard W. | Baltimore. |
| Lloyd, Samuel | New York. |
| Long, John W. | Greensboro. |
| McGannon, M. C. | Nashville. |
| McGuire, Stuart | Richmond. |
| McGuire, W. Edward | Richmond. |
| McLean, H. Stewart | Richmond. |
| Marcy, Henry O. | Boston. |
| Martin, E. Denegre | New Orleans. |
| Mayo, Charles H. | Rochester, Minn. |
| Mixter, Samuel J. | Boston. |
| Moran, John F. | Washington. |
| Morris, Lewis C. | Birmingham. |
| Morris, Robert T. | New York. |
| Mullaly, Lane | Charleston. |
| Munro, John C. | Boston. |
| Nicolson, William Perrin | Atlanta. |
| Noble, Charles P. | Philadelphia. |
| Noble, Geo. H. | Atlanta. |
| Ochsner, Edward H. | Chicago. |
| Oviatt, Charles W. | Oshkosh. |
| Price, Joseph | Philadelphia. |
| Rees, Chas. M. | Charleston. |
| Richardson, Maurice H. | Boston. |
| Roberts, W. O. | Louisville. |
| Robins, Charles R. | Richmond. |
| Rogers, Mack | Birmingham. |
| Royster, Hubert A. | Raleigh. |
| Saunders, Bacon | Fort Worth. |
| Shands, A. R. | Washington. |
| Shivers, O. L. | Marion, Ala. |
| Simons, Manning | Charleston. |
| Simpson, Frank F. | Pittsburgh. |
| Stone, Isaac S. | Washington. |
| Talley, D. F. | Birmingham. |
| Taylor, Hugh M. | Richmond. |

TOMPKINS, CHRISTOPHER . . . . . Richmond.
VANCE, AP MORGAN . . . . . Louisville.
WATHEN, JOHN R. . . . . Louisville.
WATKINS, ISAAC L. . . . . Montgomery.
WILLIAMS, J. WHITRIDGE . . . . . Baltimore.
WINSLOW, RANDOLPH . . . . . Baltimore.
WYSOR, JOHN C. . . . . Clifton Forge.
YOUNG, HUGH M. . . . . Baltimore.

Letters and messages of regret were received from a number of Fellows who were unable to attend the meeting.

*Morning Session.* The Association met in the assembly hall of the Hotel Belvedere, and was called to order at 10 A.M., by the Chairman of the Committee of Arrangements, Dr. Howard A. Kelly, who extended a very cordial and hearty welcome to the members and guests.

Dr. Guy Le Roy Hunner, a member of the Committee, called attention to the following entertainments:

Tuesday, 1 to 2 P.M., luncheon at the Maryland Club.

Wednesday, 8.30 A.M., special cars leave Hotel Belvedere for the Johns Hopkins Hospital.

Wednesday, 9 A.M., clinical session, by request of the Council in the new Surgical Building of the Johns Hopkins Hospital.

Wednesday, 1.15 to 2.15 P.M., luncheon given by the Trustees of the Johns Hopkins Hospital in the Administration Building.

Thursday, 1 to 2 P.M., oyster roast at Hotel Rennert.

Dr. Hunner thanked the other members of the Committee (Drs. Williams, Winslow, Hundley, Johnson, Young, and others) for what they had done.

He called attention to some minor changes that had been made in the program of the meeting.

Dr. Kelly then introduced the President of the Association, Dr. Geo. H. Noble, of Atlanta, who took the Chair.

The reading of papers was proceeded with.

On motion of Dr. W. P. Nicolson, the reading of papers was limited to twenty minutes, and each discussion to five minutes.

1. "Sulphate of Spartein in Surgical Practice," by Dr. Stuart McGuire, of Richmond, Virginia.

This paper was discussed by Drs. Nicolson, Winslow, and in closing by the essayist.

2. "Should the Cervix be Removed in Hysterectomy?" by Dr. J. Wesley Bovée, of Washington, D. C.

Discussed by Drs. Stone, Byford, Richardson, Boldt, Hill, and in closing by the essayist.

3. "Two Cases of Extra-uterine Pregnancy," by Dr. O. L. Shivers, of Marion, Alabama.

4. "An Improved and Accurate Method of Locating Foreign Bodies with the *X*-ray," by Dr. Robert Carothers, of Cincinnati, Ohio.

Discussed by Dr. Caldwell, and the discussion closed by the essayist.

5. "Surgical Treatment of Thyroid Disease Based on 300 Personal Observations," by Dr. Charles H. Mayo, of Rochester, Minnesota.

This paper was discussed by Drs. Crile, Winslow, and in closing by the author.

6. "Toxemia of Pregnancy," by Dr. W. M. Jordan, of Birmingham, Alabama.

7. "Report of Cases of Toxemia of Pregnancy," by Dr. D. F. Talley, of Birmingham, Alabama.

These two papers were discussed jointly by Drs. Mayo, Williams, and in closing by Dr. Jordan.

On motion, the Association adjourned until 2.30 P.M.

*Afternoon Session.* The Association reassembled at 2.30 P.M., and was called to order by the President.

8. "Conservative Method of Managing Undescended Testicle," by Dr. Joseph Price, of Philadelphia, Pa.

Discussed by Drs. Morris, Marcy, Mixter, Richardson, Finney, Hill, and in closing by the essayist.

9. "Gallstones without Symptoms and Symptoms without Gallstones," by Dr. Maurice H. Richardson, of Boston, Mass.

10. "Cholecystectomy, when Indicated; Some Operative Sequelæ," by Dr. Isaac S. Stone, of Washington, D. C.

These papers were discussed jointly by Drs. Morris, Carr, Lloyd, Marcy, Martin (Frank), Kelly, Mayo, and in closing by the essayists.

11. "Subsequent Treatment of Laparotomy Patients," by Dr. Herman J. Boldt, of New York.

Discussed by Drs. Stone, Byford, Deaver, Hall, Baldy, Kelly, Price, Chambers, McGannon, Brothers, Marcy, Balloch, and in closing by the essayist.

12. "Treatment of Senile Gangrene," by Dr. Edward H. Ochsner, of Chicago.

Discussed by Drs. Mayo, Marcy, Oviatt, Balloch, and in closing by the author.

13. "Two Simple Surgical Tricks Worth Knowing, with Instruments used," by Dr. Ap Morgan Vance, of Louisville, Ky.

Discussed by Dr. Martin.

14. "Report of a Case of Ruptured Ovarian Tumor, with Complications," by Dr. Rufus B. Hall, of Cincinnati, Ohio.

Discussed by Dr. Deaver.

15. "Hodgkin's Disease—A Type of Sarcoma," by Dr. William B. Coley, of New York.

On motion, the Association adjourned until Wednesday afternoon.

NOTE.—Wednesday morning, December 12, the members of the Association met at the Johns Hopkins Hospital, where clinics and demonstrations were given by the members of the Faculty of that Institution. (For report of these clinics, see page 458.)

SECOND DAY.—*Wednesday, December* 12, 1906.

*Afternoon Session.* The Association met in a Lecture Room, at Johns Hopkins Medical School, at 2.30 P.M., and was called to order by the First Vice-President, Dr. Stuart McGuire, of Richmond, Virginia.

The President, Dr. George H. Noble, of Atlanta, Ga., then delivered his Address. He selected for his subject, "Puerperal Thrombophlebitis of the Pelvic Veins."

On motion of Dr. Edward McGuire, a vote of thanks was extended to the President for his able and instructive Address.

16. "Direct Transfusion of Blood in the Treatment of Hemorrhage," by Dr. Geo. W. Crile, of Cleveland, Ohio.

Discussed by Drs. Dudley P. Allen (by invitation), Finney, Mixter, Morris, Welch, and in closing by the essayist.

17. "Surgical Treatment of Tuberculosis of the Kidney," by Dr. Howard A. Kelly, of Baltimore, Md.

18. "Tuberculosis of the Kidney," by Dr. Chas. P. Noble, of Philadelphia, Pa.

These two papers were discussed together by Drs. McGuire (Edward), Morris, Stone, Rodgers, Bovée, and in closing by Dr. Hunner (for Dr. Kelly), and Dr. Noble.

The President appointed as an Auditing Committee, Drs. J. Wesley Long, and Lewis C. Bosher, to audit the accounts of the Secretary and Treasurer.

19. "A Study of Vaginal Cesarean Section with Especial Reference to Subsequent Pregnancy and Labor," by Dr. James F. Moran, of Washington, D. C.

20. "Dystocia Following Fixation and Suspension of the Retroflexed Uterus," by Dr. J. Whitridge Williams, of Baltimore, Md.

These papers were discussed together by Drs. Hill, Marcy, Noble (Chas. P.), Morris, McGuire (Edward), Johnson, Long, Jordan, Stone, and in closing by Dr. Williams.

On motion of Dr. Henry O. Marcy, the Association then adjourned until 8 P.M.

*Evening Session.* The Association reassembled at 8 P.M., and was called to order by the President.

21. "A Brief Sketch of One of Baltimore's Greatest Men, Horatio Gates Jameson, M.D. His Most Valuable Contribution to Surgery: The Occlusion of Arteries by the Buried Animal Ligature." By Dr. Henry O. Marcy, of Boston, Mass.

This Address was discussed by Drs. Stone, Cunningham, and the discussion closed by Dr. Marcy.

22. "Carcinoma of the Stomach," by Dr. John B. Deaver, of Philadelphia, Pa.

Discussed by Drs. Haggard, Oviatt, Lloyd, Marcy, and in closing by the essayist.

23. "Cysts of the Omentum," by Dr. R. E. Fort, of Nashville, Tenn.

Discussed by Drs. Bovée, Long, Caldwell, and in closing by the essayist.

24. "Vaginal Section as an Operation upon the Ovaries and Tubes," by Dr. Henry T. Byford, of Chicago.

Discussed by Drs. Noble (Chas. P.), Bovée, Ill, Deaver, Hundley, and in closing by Dr. Byford.

In the absence of Drs. McMurtry, Parham, and Douglas, of the Council, Drs. A. M. Cartledge, E. Denegre Martin, and R. M. Cunningham, were appointed to fill these vacancies.

25. "Some Disjointed Observations upon our Technique," by Dr. A. M. Cartledge, of Louisville, Ky.

This paper was discussed by Dr. Deaver, after which the Association, on motion, adjourned until Thursday morning.

THIRD DAY.—*Thursday, December* 13, 1906.

*Morning Session.* The Association met at 9.30 A.M., and was called to order by the Second Vice-President, Dr. Martin.

26. "Influence of Respiration upon the Development of Chest Deformity in Scoliosis, with its Relation to the Application of Plaster-of-Paris Jacket," by Dr. Michael Hoke, of Atlanta, Ga.

Discussed by Drs. Caldwell, Ochsner, Morris, Baer (by invitation), and in closing by the essayist.

27. "Appendicitis in the Negro," by Dr. Hubert A. Royster, of Raleigh, N. C.

Discussed by Drs. Byford, Carr, Martin, Fort, Cunningham, McGuire (Stuart), Moore (by invitation), and in closing by the essayist.

28. "Aneurysmorrhaphy for Abdominal Aneurysm; Report of an Unsuccessful Case, with Autopsy," by Dr. John C. Munro, of Boston, Mass.

29. "Some Suggestions in Regard to the Surgical Treatment of Empyema," by Dr. Samuel Lloyd, of New York.

Discussed by Drs. Ochsner, Carr, Cartledge, and in closing by Dr. Lloyd.

30. "Foreign Bodies in the Urinary Bladder; Cystotomy," by Dr. C. E. Caldwell, of Cincinnati, Ohio.

Discussed by Drs. Noble (Geo. H.), Winslow, Stone, and in closing by the essayist.

The Auditing Committee, to whom were referred the accounts of the Secretary and Treasurer, reported as follows:

REPORT OF THE AUDITING COMMITTEE.

*To the Members of the Southern Surgical and Gynecological Association:*

GENTLEMEN: We, your committee appointed to audit the Secretary's and Treasurer's accounts, beg leave to report as follows:

| | |
|---|---|
| 1. The Secretary has in his hands a balance from the year 1905 . . . . . | $210.00 |
| 2. The amount of dues collected during 1906 . . | 1870.00 |
| 3. Amount of Interest received on deposits . . | 32.30 |
| Total . . . . . | $2112.30 |

We find that the Secretary has disbursed this sum as follows:

| | |
|---|---|
| 1. Paid to Dr. William Whitford, stenographer | $50.00 |
| 2. Paid by certified check of Dr. C. M. Rosser, Treasurer | 2062.30 |
| Total | $2112.30 |

There remains dues uncollected from the members $450.00.

TREASURER'S REPORT.

| | | |
|---|---|---|
| 1. There was in the Treasurer's hands as per his books December 9, 1905 | | $1462.43 |
| 2. During the year 1906 he has disbursed the following amounts: | | |
| Paid Secretary's salary | $500.00 | |
| Paid exchange on draft | 1.25 | |
| Paid stenographer | 168.81 | |
| Paid for stationery | 29.25 | |
| Paid for postage, telegrams and expenses | 49.20 | |
| Paid Secretary's and Treasurer's guarantee bond | 20.00 | 768.51 |
| Leaving balance in Treasurer's hands, December 8, 1906 | | $693.92 |
| 3. Certified check from Dr. W. D. Haggard, Secretary, December 13, 1906 | | 2062.30 |
| Total balance in Treasury | | $2756.22 |

We find that the books and accounts of the Secretary and Treasurer are correctly kept and supported by proper vouchers.

Respectfully submitted,

J. W. LONG,
LEWIS C. BOSHER,
Auditing Committee.

On motion, the report of the Auditing Committee was adopted.

The Secretary presented the following report of the Council:

REPORT OF COUNCIL.

The Council begs leave to make the following recommendations:

That Article IX of the Constitution be amended by inserting the words, "or present a paper," making the Article read: "Any member who for three consecutive years fails to attend the meetings or present a paper shall be dropped from the roll of membership."

1. That the meetings be held hereafter on the second Tuesday, Wednesday, and Thursday of November, as provided in the By-Laws.

2. That New Orleans be selected as the next place of meeting, and that Dr. E. Denegre Martin be made Chairman of the Committee of Arrangements.

3. That the resignations of the following members be accepted:

Dr. Edmond Souchon, of New Orleans.

Dr. Magnus Tate, of Cincinnati.

4. That the deaths of the following members be regretfully recorded:

Dr. L. H. Dunning, of Indianapolis; Dr. J. C. Le Grande, of Birmingham; Dr. Hugh T. Nelson, of Charlottesville; Dr. J. M. Black, of Knoxville; Dr. B. W. Taylor, of Columbia, S. C.

5. That the following-named gentlemen are eligible for membership and are hereby notified to fill the eight vacancies in the roster of membership:

Dr. Gerry R. Holden, of Jacksonville, Fla.

Dr. Carey P. Rogers, of Jacksonville, Fla.

Dr. Lucius E. Burch, of Nashville, Tenn.

Dr. John Egerton Cannaday, of Paint Creek, W. Va.

Dr. J. C. Bloodgood, of Baltimore.

Dr. Harvey Cushing, of Baltimore.

Dr. Thos. S. S. Cullen, of Baltimore.

Dr. Jno. G. Clark, of Philadelphia.

6. The Council recommends the following officers for 1906 and 1907: Dr. Howard A. Kelly, of Baltimore, for President; Dr. Rufus E. Fort, of Nashville, and Dr. Hubert A. Royster, of Raleigh, for Vice-Presidents, and the retiring President, Dr. George H. Noble, to fill the vacancy on the Council caused by the expiration of service of Dr. J. Wesley Bovée. The

present Secretary and Treasurer hold over. All of which is respectfully submitted by

GEO. BEN JOHNSTON,
J. WESLEY BOVEE,
Of the Council.
A. M. CARTLEDGE,
R. S. CUNNINGHAM,
E. D. MARTIN,
Acting Members of the Council.
GEO. H. NOBLE,
W. D. HAGGARD,
Ex-Officio.

It was moved that the report of the Council be adopted, as read. Seconded and carried.

Dr. Samuel Lloyd was asked to escort the President-elect, Dr. Kelly, to the Chair.

Dr. Kelly, in accepting the Presidency, said: I appreciate more than I can tell you the honor and privilege it is going to be to preside over this Association during the coming year.

It seems to me there are one or two distinctive things in this Association for us to attend to during the coming year, namely, the scientific work and Fellowship. We are all interested in the scientific work, and we do not need any particular stimulus or encouragement. We cannot, however, pay too much attention to Fellowship, kindly feeling, professional amenities, and affectionate relationships all over this country, which this Association has done so much to promote. If I can do anything to bring the men of the South more together and to get men from the North to come down and join with us, and to make warmer friends of members of the medical profession throughout the country, that will be my great object. I regard this as of more importance in these days than simply the reading of scientific papers. I shall be happy in trying to work both of these ends in connection with the labors of the Chair during the coming year. But this can only be done with the coöperation of the officers and members, and in speaking of this, after all, we feel that the real President (referring to the Secretary) sits to my left. (Applause.) Again, gentlemen, I wish to thank you very much for the great honor you have conferred on me. (Applause.)

31. "Normal Involution of the Appendix," by Dr. Robert T. Morris, of New York.

Discussed by Drs. Lloyd, Caldwell, Morris (Lewis C.), Noble (Geo. H.), Ill, and in closing by the essayist.

32. "Splenectomy, Indications and Results; Report of Five Successful Cases," by Dr. W. P. Carr, of Washington, D. C.

Discussed by Drs. Stone, Morris (Robt. T.), Morris (Lewis C.), and in closing by the essayist.

33. "Neoplasms of the Hyoid Bone," by Dr. Randolph Winslow, of Baltimore, Md.

Dr. John C. Wysor, of Clifton Forge, Va., presented an interesting case and specimen of extensive necrosis of the skull.

The following papers were read by title and ordered published in the proceedings:

1. "Final Results of Operations for the Cure of Epispadias," by Dr. Walter B. Platt, of Baltimore, Md.

2. "A Simple Method of Treating Umbilical Hernia in Infants," by Dr. William Perrin Nicolson, of Atlanta, Ga.

At this juncture, Dr. Samuel J. Mixter, of Boston, arose and said: Before adjourning, Mr. President, it seems to me it is fitting to say something about this meeting and of those who have made it such a great success. I am told that this is the largest meeting the Association has ever had; that there have been 83 members out of 195 present. In addition to the high character of the papers read, there were more members present to read their papers than ever before. I understand that only 6 out of the 44 members listed on the program to read papers were absent. What can we say for those who have made this meeting such a great success, our hosts, especially the Committee of Arrangements, and particularly Dr. Kelly, the Chairman, and Dr. Hunner, who have done so much for us. Then the Trustees of the Johns Hopkins Hospital, who did so much for us yesterday; and the President and officers of the Society, who arranged the program so efficiently. I move, therefore, Mr. President, the following resolution:

*Resolved.* That the thanks of the Association be extended by rising vote to the members of the Committee of Arrangements, to the Trustees of the Johns Hopkins Hospital, to the members of the medical profession in Baltimore, and to the ladies for the courtesies they have shown us and for the admirable manner in which they have entertained us and our wives.

The resolution was seconded by several and unanimously carried.

On motion, the Association then adjourned, to meet in New Orleans, La., on the second Tuesday in November, 1907.

W. D. HAGGARD, M.D.,
Secretary.

The Association does not hold itself responsible for the views enunciated in the papers and discussions published in this volume.

W. D. HAGGARD, M.D., *Secretary*,
NASHVILLE, TENN.

ANNUAL ADDRESS OF THE PRESIDENT.

# SURGICAL TREATMENT OF PUERPERAL THROMBOSIS OF PELVIC VEINS.

By George H. Noble, M.D
*Atlanta, Ga.*

In making this plea for an early diagnosis and prompt surgical interference in one of the most serious forms of puerperal diseases, I will ask your indulgence for a short time only, as there is very little literature upon the subject, and no one seems to know very much about it.

It will be observed that this paper studiously avoids any reference to the pathology, etiology, or medical treatment, because its object is to direct attention to the surgical aspect of the question. Neither does the white or non-infected clot receive consideration, because of its tendency to recover under medical attention.

Of the 32 cases[1] of puerperal fever, the infection was divided as follows with reference to location:

| | |
|---|---|
| Metrophlebitis | 11 |
| Metrolymphangitis | 4 |
| Abscess in parametrium | 8 |
| Thrombophlebitis (suppurating) of broad ligament | 2 |
| Suppurative peritonitis | 7 |
| Deep laceration of vagina | 7 |

Thus, of the entire number, there were 13 cases of pelvic phlebitis, 40.6 per cent., 2 of which were in the large

[1] Amos of the Anatomo-Pathological Institute of Recklinghausen.

veins of the pelvis, 6.2 per cent. If thrombus of the pelvic veins occurs in 6 or 7 per cent. of puerperal infection, the mortality being high, considerable saving of life may be made by early recognition of the disease and prompt interference. I believe the showing should be greater, for the number of cases coming under my observation does not correspond to the percentage given in the above list. And because in abdominal sections made for other diseases, thrombi are unexpectedly encountered, or perhaps overlooked, on account of the importance of the lesions for which these operations are performed, and because many puerperal cases terminate fatally with the true conditions unrecognized, these things cause me to think that the occurrence of thrombosis and its mortality are not fully appreciated.

Thrombosis may begin early, but usually the symptoms are not clearly defined until late in puerperal state, making a distinction between it and other clinical manifestations of puerperal sepsis, excepting intramural abscess of the uterus. But, exceptionally the symptoms may appear earlier. In Blot's case[1] the chill came on the third day, and on the ninth the thrombus had advanced to such an extent resection of the left side of the uterus and the corresponding broad ligament was required.

It has its origin in infected clots in the veins at the placental site, and if these lead to the uterine, ovarian, or round ligament veins, the coagulations will extend along the course of these vessels; but, if they follow smaller vessels that lead to the peritoneal surface of the uterus, they may result in abscesses when the infection becomes more or less sterile.

The disease is often complicated with other septic conditions, such as involvement of tubes, ovaries, suppuration in the parenchyma of the uterus, para-uterine tissues, or the clot itself may be permeated with pus, the latter escaping directly into the circulation.

Thrombi may extend from the pelvic to the common

[1] American Journal of Obstetric Medicine, 1905, p. 368.

iliac veins, and grow until the inferior vena cava is filled, but the size of the coagula does not always correspond to the gravity of the symptom. Intense infection of the small thrombus may cause more marked symptoms than a larger clot of less virulence. This was illustrated in a case of mine at the Grady Hospital. The clot was confined to the vein of the right round ligament and was only three-sixteenths of an inch in diameter and two inches in length. The symptoms were violent, but were immediately and completely relieved by removal of the vein. Upon the other hand, another case with involvement of the pampinaform plexus, uterine and ovarian veins of the right side, the symptoms did not differ materially from a mild case of puerperal infection of uterine mucosa. Cases like this are doubtless often mistaken for the so-called idiopathic thrombus, there being little or no constitutional disturbance.

The symptoms, according to Heiderman, are headache, slight irregular rise in temperature, small frequent pulse (gradual but steadily increasing), dysuria and ischuria, sudden pain in side, pains in bending hip (in femoral vein under Poupart's ligament), lassitude, and attack of vertigo on leaving bed. Howard[1] says "temperature and pulse rise rapidly; rigors supervene, followed by profuse sweating; the tongue is dry; appetite is lost; and there may be delirium. To these symptoms are soon added those of pyemia, rapid oscillations of temperature, rigors, sweating, and disseminated suppurations." Mahler's symptom is gradually and steadily increasing pulse (climbing). In bad cases it is a most important one. Vany[2] states that the pulse and temperature curves vary. The pulse may go up in successive bounds as the temperature comes down. Each forward leap of the pulse curve corresponds to a new extension of the clot.

In all cases reported, the chill is a prominent feature. It usually comes on in the second or third week, showing the

[1] Howard's Hunterian Lectures.

[2] Ann. de Gyn. et d'Obst., August, 1906.

tendency to slow extension and pathogenic invasion of clot, but may be deferred, as in one of Duret's cases, to come on the fortieth day, or perhaps later. If the external or common iliac vein is involved, there will be swelling of the leg, and the duration of the disease may be prolonged as much as seventy-four days or more. But there are cases of very short duration which are sometimes masked by complications, and may be entirely overlooked on account of the absence of suggestive signs and symptoms.

While the above symptoms are commonly met with in the disease, they represent cases that are influenced more or less by process of suppuration and, therefore, differ materially from those that characterize uncomplicated and non-suppurating but infected thrombi. I have observed that the latter give the following chain of symptoms:

The disease may or may not be preceded by signs and symptoms of slight septic disturbances for the first week or two. These manifestations whenever present become less marked and may entirely disappear, leaving the patient apparently convalescent. In the second or third week the first pronounced symptom is the chill, which is well marked if infection is intense. This is accompanied by rapid pulse and sharp rise of temperature to 103° or 105°, terminating in rather rapid decline to or near normal. These symptoms usually occur once in twenty-four hours, but may be repeated in shorter periods of time. In the interval, the patient is apparently in a normal condition, except that she is left weaker after each attack. The change way be slight, consequently one may be deceived into the belief that each chill will be the last before convalescence begins; but on the contrary, the pulse shows gradual loss in tension and volume and an increase in rapidity (climbing).

The pelvis may be negative on examination if the thrombus is small, but when large or late in the disease, they feel like tortuous cords to the fingers.

When the infection extends through the walls of the vessels

the para-vascular tissue may be hard and nodular, the general direction of the exudate taking the course of the vessels, which is a material aid in diagnosis. Such exudate are not markedly sensitive or painful to pressure in bimanual examinations.

Diagnosis of uncomplicated puerperal thrombosis of the pelvic veins is made by taking into consideration the above symptoms with the result of careful pelvic examinations. The value of the latter consists mainly in the ability to exclude other conditions. If, in the examination of the pelvis, the process of involution of the uterus appears not to have been materially interfered with, that is, if the uterus is firm and reasonably small for the period of puerperium, if the cervical canal is not patulous, and no unhealthy discharge escaping from it, it is reasonable to suppose that the uterus is not involved. Infection of the uterus may then be excluded, and the same may be said of the fallopian tubes and ovaries when they are not palpable or buried in exudate.

Thrombosis due to laceration of the vagina is comparatively rare. This source of origin, therefore, can usually be disregarded, especially if the lesion is slight in extent; but if the tear extends high up in the vagina or deep into the ischiorectal fossa, the lateral vaginal plexus of veins may become involved, and when it does occur they are covered up at an early stage of the disease by exudate, and in the place of cord-like vessels being felt on the side of the vagina, a nodulous ridge may lead up to the base of the broad ligament. When this condition is found, if other parts of the pelvis are free, localization of the thrombus in the lateral vaginal veins can be safely determined.

In differentiating between thrombosis and intramural abscess of the uterus, uncomplicated by peritonitis and adhesions, small rounded, flat elevations upon the surface of the uterus may be felt in patients with very thin or relaxed abdominal walls. Such cases present the ordinary septic pulse and temperature curves, and do not have the distinct

interval of non-suppurating thrombus. When complicated by adhesions, or diseases of the appendages with tense abdominal walls, exclusion of intramural abscess and detection of tortuous veins are extremely difficult.

Suppurating thrombosis will very likely be overlooked when complicated with peritonitis, pus tubes, etc., unless symptoms of pyemia supervenes, or the pulse show a tendency to become climbing in character. These are suggestive of the true condition.

The treatment is mainly surgical,[1] for when diagnosis is made most of them are well advanced into the third and fourth week, which is ample time to test temporizing treatment, and mild cases either show signs of improvement earlier or go unrecognized all together.

Any puerperal case with pelvic lesions, variable temperature, and climbing pulse of three to four weeks' duration without signs of improvement, justifies an operation of some kind, especially if the uterus proves negative as the source of trouble. If a mistake is made in diagnosis, and the locus infectus is found in the fallopian tubes, or abscesses in other parts of the pelvis, no surgical error is committed for they too are in need of serious attention.

There are several plans of procedure: First, excision of thrombosed veins; second, ligation of thrombosed veins; third, hysterectomy. In addition to these, attention to the complicating conditions must be given.

Some bad cases with small but intensely infected clots may be quickly relieved by removal of small veins. Upon the other hand, cases of apparently mild type may have great development of veins requiring extensive excision; therefore, the magnitude of this operation cannot be determined beforehand. Prolonged cases with extensive envolvement require trying operations, and are less able to stand them.

[1] In this I do not include all cases of phlegmasia alba dolens, for so often it does not depend upon local (or pelvic) lesions, but is due to other causes in the vascular system.

Guicciardi's objection to excision is the liability of reinfecting the wound by soiling the parts with the clots. How great the liability to this accident is, I cannot say, but in two of my cases in which the veins were lacerated by digging the clots out of the broad ligament with the fingers, the temperature range for two or three days was quite irregular and reached high marks at short intervals (103° to 105°), but some of this temperature may have been due to the extensive surface involved. Each of these had numerous abscesses in the uterus, ovaries, and pus collections between the coils of the intestines, besides the omentum and intestines were softened and adherent from the umbilicus downward. As the chill was not repeated in either case it is very likely that soiling the wound with clots had little to do with it. This may be avoided, however, by double ligation and cutting between to prevent coagula escaping from the ends of the vessels. Should the vessels be lacerated, as in the cases mentioned, a drain may be inserted to protect the peritoneum.

A very few cases have been reported. The number of excisions in Guicciardi's collection is too small for just conclusions to be drawn, and the conditions may vary so much that the methods may not have had equal advantages. Even the addition of five cases (one by Blot and four of my own) so modified the mortality that the showing for this method of operating quite surpasses ligation. It is theoretically the most surgical, but it must have its limits, particularly in cases that cannot stand long operations. In early cases this method is to be preferred, especially if the clot is confined to the veins of the true pelvis.

Ligation of the veins is not satisfactory, especially if the clots become infected with pyogenic cocci. Pus in a large vein shut out from the circulation by a ligature only, is a hazardous condition. If the ligature is of soluble material it may disappear, or loosen too soon and let a flood of pus into the general circulation. Neither is it a sufficient barrier to the further progress of infecting micro-organisms. They

can pass beyond the obstruction by permeating the walls of the vessels and infect the new clot on the proximate side, and thus renew the process of thrombophlebitis.

Hysterectomy is extremely dangerous in this condition. In the first place, it is often delayed too long to successfully remove all the infected centres. When thrombosed vessels are confined to the uterus it has some chance of success, but propagation through the vessels into the broad ligament extends the infected field into the pelvic veins; and at times the uterus may not be at fault, for the thrombi may lie exclusively in vessels outside of that organ. Removal of the uterus in such circumstances is worse than useless, as it would leave the thrombosed vessels undisturbed. Upon the other hand, excision of the diseased veins without hysterectomy is promising except in extreme cases.

In the summary of the small number of cases accessible, the results are as follows:

| | Cases. | Deaths. | Mortality. |
|---|---|---|---|
| Resection of veins . . | 7 | 2 | $28\frac{4}{7}$ per cent. |
| Ligation of veins . . . | 9 | 4 | $44\frac{4}{9}$ " |
| Hysterectomy . . . . | 40 | 25 | $64\frac{1}{4}$ " |

From this, I venture the assertion that until further experience shall have worked out the solution of this problem, we must accept the opinion that early recognition of septic thrombosis of the pelvic veins and prompt excision are the best method of surgical relief we can offer our patients in this disease.

# SULPHATE OF SPARTEIN IN SURGICAL PRACTICE.

By Stuart McGuire, M.D.,
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Like most surgeons I devote little time to the study of the therapeutic action of drugs. Patients who are referred to me have usually exhausted the resources of materia medica, and in my practice I rarely have occasion to employ medicinal agents other than the well-known anesthetics, antiseptics, purgatives, and tonics. I believe, however, I have accidentally discovered in sulphate of spartein a valuable remedy for the prevention and treatment of post-operative suppression of urine.

I do not know whether my experience coincides with that of other surgeons, but it is a fact that in the last five years I have lost more cases from post-operative suppression of urine than from all other causes combined, and this despite the almost routine use of chloroform as an anesthetic.

The cases have usually been those with pre-existing nephritis from sepsis or cholemia. Shock has not apparently been a factor, as the condition would not develop for twenty-four or thirty-six hours. A patient operated on for retention of urine or for jaundice due to obstruction of the common duct, would do well for one or two days, and just as he was thought to be out of danger there would come the news that he was passing no urine. He would become restless, then listless; would develop a stupor which would rapidly deepen into coma, and would die with all the symp-

toms characteristic of uremia. In the treatment of this condition I tried water by mouth, under the skin, and in the rectum; hot packs and vapor baths; cups and counter-irritants; strychnia, digitalis, and nitroglycerin; calomel and saline purgatives, and in one case stripping the kidney capsules, with uniformly bad results.

Two years ago I began empirically the use of sulphate of spartein, and I now have the record of six cases in which I am sure the drug was the means of saving the patient's life.

I will not occupy the time of the Association by reading a dissertation on spartein which I would, of course, have to copy from a text-book. Its therapeutic effect is to increase the blood pressure, make the pulse slower and stronger, and act as a powerful diuretic. Its action is manifest in thirty minutes after administration and lasts for four to six hours.

I believe the reason why the value of sulphate of spartein is not more widely recognized is because authorities advise its use in doses so small as to be worthless. To get results it must be given hypodermically in from one to two grains, repeated every three to six hours. When so employed I have repeatedly seen it pull up a runaway heart and set in motion a pair of stalled kidneys. Its use should not be delayed until suppression of urine is already in existence, but it should be prescribed as a prophylactic as well as a curative agent.

I do not mean to claim it is a specific, or that it should be employed to the exclusion of other measures, such as purgatives, transfusions, and hot packs. I do believe, however, from actual experience that it is preferable to the drugs of the digitalis type in rapidity of action, ease of administration, and what is more important—efficiency of results.

## DISCUSSION.

Dr. William Perrin Nicolson, of Atlanta.—Sulphate of spartein is a very valuable drug. I have used it somewhat after the manner Dr. McGuire has done, and as a heart tonic in preference to digitalis. It combines therapeutically the virtues of being a powerful heart tonic and, at the same time, dilating the capillaries, like atropin, as well as the kidneys in the same way.

An important point which the essayist brought out is with reference to the dose that should be given. Usually manufacturers put up this drug in $\frac{1}{10}$ grain doses; and druggists will not put it up in doses of 2 grains unless we personally see about it. Its value depends on large doses. It is an excellent heart tonic. In a case of irregularity of the heart there is nothing in my judgment which surpasses sulphate of spartein. We know how hopeless these cases of suppression of urine are when they start. I recall one patient who went forty-seven hours without passing any urine, but finally came around. It seems to me, that this is a very valuable drug, although the profession is practically ignorant of its therapeutics. My attention was called to the drug by a medical friend some years ago for controlling irregular pulse, and it is valuable if given in sufficiently large doses. The ordinary doses are absolutely inert.

Dr. Randolph Winslow.—I would like to ask the essayist how large a dose he gives?

Dr. McGuire.—Two grains.

Dr. Winslow.—How often repeated?

Dr. McGuire.—From three to six hours.

# THE COMPARATIVE ADVANTAGES AND DISADVANTAGES OF HYSTERECTOMY AND REMOVAL OF THE BODY OF THE UTERUS.

By J. Wesley Bovée, M.D.,
*Washington, D. C.*

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As the study of uterine neoplasms progresses and the after-history of patients operated on for such growths is more carefully considered we are confronted with the fact that comparatively frequent malignant disease of the cervix uteri is discovered after the body of that organ has been removed. It is found that an important proportion of cases of tumors thought to be simple fibromata or myomata, have in reality taken on malignant degeneration or are complicated by a malignant degeneration of the corporeal endometrium. Carcinoma of the cervix has been detected within a few days after removal of the body of the uterus, showing clearly it existed at the time of the supravaginal amputation. In innumerable instances removal of the cervical stump has been done at the same sitting, prompted by discovery of malignancy in the uterine body just removed. Not a few observers have seen cases in which fibromata have existed for years, the patients declining operation, and cancer of the cervix develop subsequently with a fatal result. I have seen two such cases. One of them I saw first in consultation in 1886, and again a few months before her death in 1902. The fibroid mass extended to about two inches above the umbilicus, remaining practically stationary throughout the sixteen years. No doubt exists as to the necessity for the removal of the

cervix with the body in malignant disease of the uterus in its early stages. The relative facility of supravaginal amputation of the uterus compared with total extirpation has created a very decided preference in favor of the former procedure. If this prejudice is to be overcome, it will be done only by overwhelmingly proving the necessity therefor, a result I cannot hope to effect by what I am to offer in this paper. I can only invite the attention of this honorable body of surgeons and gynecologists to the facts connected with the proposition. To do this several points suggest themselves for consideration.

They are:

1. The relative frequency of cancer of the cervix with or without fibroids of the uterus.

2. The relative frequency of cancer of the body of the uterus independently and as a complication of fibroids in that organ.

3. The etiological relation of fibroids of the body of the uterus to malignant diseases of it.

4. The causative relation of fibroids and cancer of the cervix.

5. The existence of other conditions, such as infections, catarrh, or laceration of the cervix, as a possible indication for extirpation of the cervix.

6. The malignant degeneration of uterine fibroids.

7. The difficulty of ascertaining at the time of or before operation whether such degeneration is present.

8. Malignant degeneration of the cervix subsequent to removal of the uterus.

9. The age of the patient and her family history as to malignancy.

10. The comparative mortality of supravaginal amputation and total excision of the uterus.

11. The relative ease of the two operations.

12. Fatal points in the technique of the former in addition to prompt removal of the cervix.

13. The dangers to surrounding structures in, and special complications of hysterectomy.

That cancer of the cervix occurs more frequently with uterine fibroids than without them, has not been proven. This subject has received careful study by a few observers, all of which have declared no practical difference in such frequency exists. The same is not to be said of cancer of the body of the uterus.

Admitting that the presence of fibroids in the uterine body has no influence with development of cancer of the cervix, valuable information from statistical tables on the complications of fibroids is available to demonstrate the remarkable increase in the frequency of cancer of the body of the uterus as compared to its existence in the cervix as such complications.

The percentages of cases of cancer located in the body and the cervix are generally estimated by many competent observers as 10 per cent. and 90 per cent.* It is likewise conceded that the ratio is markedly changed in the presence of uterine fibroids. C. P. Noble found in a study of 2274 cases of uterine fibromata that cancer of the body was found in 45, and of the cervix in 16 of them. This changes the percentages to 74 in the body and 26 in the cervix. The proportion of cases of cancer of the body of the uterus to those of cancer of the cervix is thus shown to be increased 26 times in the presence of fibroids. J. B. Sutton[1] had in 500 cases of fibroids of the uterus 63 that were more than fifty years of age. In 8 of the 63 cancer of the body of the uterus was microscopically proven and in 2 primary cancer of the Fallopian tube was

* Williams[9] found but 4 cases of cancer of the body of the uterus to 156 involving the cervix in 160 consecutive cases of uterine cancer coming under his observation. Winter[10] in studying 2331 cases occurring in Germany finds the proportion was 1 to 15. Robb[3] found that of 42 cases of cancer of the uterus admitted to Lakeside Hospital 6 of them were of the corporeal variety—a proportion of 1 to 6. Noble suggests the proportion of cases of cancer of the uterus to those of cervical cancer as about 1 to 10.

noted. He has had 23 cases of cancer of the body of the uterus, in 8 of which fibroids were present as a complication. Piquand (quoted by Sutton[2]) collected 179 cases of cancer of the body of the uterus complicated by fibroids, and had in 1000 cases of fibroids cancer of the body of the uterus as a complication in 15 cases.

In Noble's report of 2274 cases of fibroids, sarcoma was a complication in 34 cases, making a grand total of 95 cases, more than 4 per cent., of malignant disease existing as a complication.

From these figures one cannot avoid recognition of the existence of a strong influence in fibroids of the uterus as a cause of malignant degeneration of that organ.

The etiological relation between fibroids of the cervix and cancer of the cervix has not been established. Nor am I aware that any systematic study of this subject has been projected. Fibroids of the cervix are so rare and cancer of this portion of the uterus so common, comparatively speaking, that probably this relation, if it exists, is quite trivial. From analogy it might be considered as a working hypothesis for such investigation. That fibroids of the body exert any such influence on cancer of the cervix has not been even conjectured.

There are other conditions of the uterus that may exist, even quite commonly, that may have a deciding influence on the advisability of removal of the cervix when the body is to be removed. These are notable laceration, marked cystic degeneration and marked hyperplasia of the cervix, infections of the uterus, especially tubercular, and chronic catarrh of either the cervix or the whole endometrium. To leave in a badly degenerated cervix after removal of the body of the uterus is to invite malignant degeneration of it.

Malignant degeneration of uterine fibroids is a subject of great importance and is being studied very thoroughly. The time has arrived when removal of uterine fibroids does not end the responsibility of the surgeon. He must know the

condition of such uterus and growths both macroscopically and microscopically. We are very deeply indebted to C. P. Noble, Frederick, Hunner, Scharlieb, MacDonald, Winter, Webster, and a few others for emphasizing this subject and offering such positive data. They have convinced the profession that early degeneration of uterine fibroids, benign or malignant, cannot be determined by symptoms, any more than uterine cancer can be so recognized. Before operation the discovery of malignant degeneration of fibroid uterine tumors and of the mucosa adjacent to them is in a majority of cases impossible.

Even uterine scrapings may not include the portion of the mucosa located on shelves caused by submucous fibroids projecting markedly into the uterine cavity. It is in such portions that the mucosal irritation and hypertrophy is most marked and, according to many observers, the malignant growth most apt to begin. Even at the time of operation the discovery of malignant degeneration of uterine fibroids cannot be a certainty. Indeed such degeneration must be marked, well progressed, for macroscopic evidence to be apparent. And even the mucosa of the fibroid body of the uterus may be carcinomatous without naked-eye appearance of it. Certainly adenocarcinoma of the cervix may exist in the presence of uterine fibroids without attracting attention. Such cases have been reported by Lewis,* Sutton, and many others, in which a second operation a few days after removal of the uterine body has been done to remove the cervix discovered to be the site of carcinoma, or when a study of the corporeal endometrium has proven it to be cancerous.

Certain it is that the careful inspection of the tumors and endometrium should be made as soon as the specimen is removed from the body. This may be done by an assistant who may hold it convenient for careful survey by the surgeon before finishing the toilet of the peritoneum. This idea has received strong emphasis from surgeons who, like Cullen,[5]

* Loc. cit.

have had a continuation of a malignant process from an operation unnecessarily narrow as a result of failure to carefully inspect a specimen of uterine tumor at the time of removal. In some clinics the aid of the microscope is employed during the operation. This must be of signal advantage as a routine procedure and particularly so in special cases. It, however, should not, in my judgment, act as a barrier against radical operation for growths clinically suspicious of malignancy. Nor should it be a substitute for thorough systematic examination in the laboratory.

Malignant degeneration of the cervix after removal of the uterine body no doubt occurs quite commonly, though Henrotin,[6] in a discussion of this subject in the American Gynecological Society, lays particular stress upon the liability of clinically confounding granular tissue, from deficient blood supply, with cancer of the cervix. He relates his experience in one such case in which the pathologic diagnosis of benign granular tissue had been clinically diagnosed as cancer and removal of the cervix performed. In the same discussion of Currier's timely paper, Newman exhibited a specimen and slides of a carcinoma that developed seven years after removal of the body of the uterus for multiple fibroids. The cancer was limited to the stump. C. P. Noble also referred to the 16 cases reported by Winter and two of his own. Manton reported a case of sarcoma of the cervix discovered three months after removal of the body of the uterus for a large growth having the clinical appearance of a fibroid. Peraire and Bender[7] report a case of a woman of fifty-two years in which they removed the body of the uterus for fibroids that microscopically contained no cancer. Four to six months after operation cancer of the cervix developed. Richelot[8] reported 3 cases of this kind in 13 cases of hysteretcomy for fibroids.

The age of the patient and the family history as to cancer deserve consideration. As cancer of the uterus occurs most frequently after the thirty-fifth year the advisability of re-

moval of the cervix would seem to be greatest after that age. Suttons* found cancer of the body of the uterus verified by microscopic examination in 8 of 63 women beyond the fiftieth year that were operated on for uterine fibroids. Thirteen of them were beyond sixty years of age. If the family history shows lineal ancestors have been afflicted with cancer an additional reason for removal of the cervix is thus furnished.

In years past the mortality of supravaginal amputation was less than that of hysterectomy. The technique, cleanliness, and experience in operating were incomparable to what they are now. Moreover, the complete operation was reserved for the severest cases—those complicated. Necessarily the mortality rate was higher.

The vaginal route at that time had not been so amplified and had not become so universal as at present. Naturally, the cervix is removed whenever the body of the uterus is removed by that route. Even suppurative adnexal disease furnishes an indication for vaginal panhysterectomy. So that removal of the cervix is considered of very little moment as regards modification of the mortality rate whenever removal of the body of the uterus is to be done, whether by the abdominal or the vaginal route. In my judgment there is practically no difference in the mortality of the total and the subtotal operation. I am not aware of any reliable statistics of latter-day surgery to prove anything either way in this matter.

In the work of the tyro I readily admit that by the abdominal route the amputation will be easier than the total extirpation. Even to the experienced pelvic surgeon exceptional cases exist in which total extirpation will be a trifle more difficult. But is the surgeon to make the paramount issue his ease in operating, or will the welfare of his patient be of first consideration?

I suggest that, as far as possible, the selection of the partial

* Loc. cit.

or the complete hysterectomy should be made before the endometrium or the interior of tumors is exposed. Otherwise amputation, preliminary to removal of cervix, offers grave danger of infection of the peritoneum. The danger of such contamination is one of the fatal errors of myomectomy. If malignant disease be present in the tumor or endometrium, removal of the body and of the cervix separately unnecessarily exposes the peritoneum to contamination. Complete extirpation from above certainly increases the danger of injuries to bladder and rectum. Beyond this no special danger exists in complete hysterectomy as compared to the partial operation. Owing to bladder displacement in the complete operation, probably post-operation cystitis is more common. Surely the vaginal support is lessened by it unless the broad and round ligaments are sutured to the vaginal wall.

Having presented these facts, I am prompted to suggest that complete extirpation of the uterus is not an operation, even by the abdominal route, that necessarily taxes the patient's vitality to an extent appreciatively greater than does removal of the body alone, notwithstanding an impression has been inherited from the period of relatively bad surgery that combined inexperience and bad technique to overwhelm the patient. By the vaginal route it is a much easier and more commonly practised operation than removal of the body and not the cervix. Injuries to the ureters and bladder now occur nearly entirely in the work of the incompetent surgeon. The objections to removal of the cervix with the body because of weakening of the vaginal roof and shortening the vagina are practically nullified by suturing the round and broad ligaments to the vaginal walls, and the fact that in employing the vaginal route these objections are forgotten. We have to consider of great importance, that about 6 per cent. of cases of uterine fibroids have malignant disease either complicating them or developing later in the stump, and that in a con-

siderable proportion of them the complication is not discovered until after operation. In these cases the patients have been endangered by possibility of contamination of the peritoneum, by severing the uterine tissue and canal, and stimulation of the stump by traumatism that may, perhaps, in itself produce a rapid extension of the malignant disease. Entire removal affords better drainage facilities should they be required, and brings the pedicles within reach by the vaginal route should their inspection or treatment be deemed necessary.

As against the removal of the cervix when the body of the uterus is removed, we have only the abdominal route to be considered. The comparative ease of the amputation is of moment only in critical cases. The cosmetic effect to young women deserves more consideration. Oftimes retention of the portio vaginalis is deemed by them to be of great value. It would seem, then, that in the young woman without a family history of malignant disease, and in the absence of uterine growths, the cervix should not be removed if the body is to be removed by the abdominal route. In very critical cases in which the body of the uterus is to be removed by that route, even in the presence of tumors, it will occasionally be advisable not to remove the cervix. In practically all other cases the cervix should not be saved, and this injunction would seem to be the stronger in old women, particularly if evidence of uterine growths be present.

Bibliography.

1. Sutton, J. B. Jour. Obst. and Gyn. of the British Empire, 1906, x, 1 to 5.

2. Piquand. Les degenerescences des fibro-myomes de l'uterus, Paris, 1905, 8°, p. 425.

3. Robb. Amer. Gyn. and Ped., 1904, xvii, 614.

4. Lewis. Amer. Jour. Obst., 1905, lii, 484.

5. Cullen. Jour. Amer. Med. Assoc., 1906, xlvi, 695.

6. Henrotin. Surg. Gyn. and Obst., 1906, iii, 171.

7. Peraire and Bender. Bull. et mém. Soc. anat. de Paris, 1904, lxxix, 774.
8. Richelot. Ann. de gyn. et d'Obst., Paris, 1900, liv, 214.
9. Williams. Uterine Tumors, etc., London, 1901.
10. Winter. Zeit. f. Geb. u. Gyn., 1906, No. 1.

## DISCUSSION.

Dr. I. S. Stone, of Washington, D. C.—The discussion of this subject has taken a wide range in the gynecological societies of Paris, and Richelot in his paper, to which Dr. Bovée has called our attention, gave an immense amount of data of importance on this subject. It is surprising to observe how many surgeons have recorded themselves in favor of what is called subtotal hysterectomy, which in this country we call Baer's operation.

In regard to the question of malignancy of the stump, that is a matter which is being agitated so generally that, like Banquo's ghost, it will not down, and we may still consider it. As we all know, it has been found by the majority of those who have investigated the subject that the embryonic remains of the Müllerian ducts, or some other embryological abnormalities, are the entering wedge of the malignant cells into the body of the uterus or into the tumor itself. It has been my good fortune to have saved two patients at subsequent operations, who had subtotal hysterectomy performed with the stump remaining, which was found to be malignant. Subsequent operation was performed, and, I am happy to say, those patients are alive. One was a case of sarcoma.

The most important thing, it seems to me, is to call attention to the fact that hysterectomy is not merely to be considered from the standpoint of mortality, but, as has been wisely said, from the point of view of morbidity. I think there are quite a number of surgeons who are perfectly satisfied with the mortality of the Baer operation of leaving the stump. Their immediate results are satisfactory, but they should also be alive to the importance of a smooth recovery and the question of total or subtotal hysterectomy is not fully settled.

As to antiseptic precautions, I think any surgeon ought to apologize if he talks about antiseptic precautions any more in these days. It is distinctly understood that we must use

the proper precautions, otherwise we are not entitled to speak on this floor. At the same time, embolism does follow in the wake of those who do hysterectomy. I doubt if there is a man here who has not been very much astonished to find that a patient who was doing apparently well, has suddenly gone hence. I have unfortunately suffered that way, and I do not know of anything which is more demoralizing. We may have taken pains to inform the friends and family of the patient that she is sure to get well; and yet all of a sudden there is a change of scene. What causes some of these accidents? I do not believe we will ever get rid of some of these dangers, but I am convinced that leaving the cervix is one of the reasons why we have a certain number of deaths or mortality from the subtotal hysterectomy operation.

Dr. Henrotin's name was mentioned in connection with this subject. I do not know whether the essayist mentioned other names or not, but all surgeons know that necrosis occurs more frequently in the stump of a tumor than in the other tissues, such as the connective tissue, the peritoneum, or whatever muscular tissue may remain in the broad ligaments. Having had fairly good results with subtotal hysterectomy, so far as the death list is concerned, I have been doing these operations with a little modification, and remove all or part of the cervix to give perfect drainage. I began by introducing a drainage tube in the dilated stump and leaving it there to drain the subperitoneal space immediately under the bladder and peritoneal flap. It has been astonishing, as it has been gratifying, to see the temperature curve reach very closely to the normal in these cases by pursuing such a course.

Dr. Henry T. Byford, of Chicago.—I do not believe that we should make it a rule to take out the cervix in all cases. In operating upon young persons we should not do so without good reason, for the resulting contraction of the vagina is liable, sooner or later, to interfere with marital relations. In senile cases the extirpation of the cervix is sometimes so difficult that it materially increases the danger of the operation.

My habit has been to employ a procedure which can be easily carried out, no matter what the conditions are. I cut a wedge-shaped piece from the cervix, and extend the wedge downward in a conical shape until I not only include the entire cervical endometrium and contiguous cervical

tissue but, if desirable, a large portion of the vaginal covering of the cervix. By drawing up the cervical cone the cervical shell can be partly everted so that the mucosa of the vaginal portion is drawn up into the cervical canal and, for the time being, becomes a part of the core that is being cut out. Removing this, the mucous membrane, removes the nidus of future carcinoma. If carcinoma be subsequently found in the extirpated mucosa, a subsequent operation can be performed which can be made more radical than a routine extirpation of the cervix in connection with abdominal hysterectomy for fibroids is apt to be.

I wish to call attention to one point in the paper which has been a lesson to me. I did not realize the frequency of the occurrence of carcinoma of the endometrium in connection with myoma, and the necessity for taking it into consideration. There are, however, two reasons why this fact does not justify the substitution of hysterectomy for possible myomectomy. First, we can warn the patient to report to us the moment she notices any uterine discharge or other evidence of uterine disease, and thus discover the cancer early; and we know that early hysterectomy for carcinoma of the endometrium nearly always cures the patient. Secondly, the cases of myoma uteri in which myomectomy can be performed are numerous and the coexistence of carcinoma proportionately rare. In other words too many uteri would be sacrificed for the amount of danger involved when we consider the good chance for a cure by a subsequent hysterectomy even when carcinoma does appear.

Dr. Maurice H. Richardson, of Boston.—This subject from the point of view of the general surgeon is extremely interesting. I have been unable to prepare any detailed statistics of my own experience in connection with it; but I have quite decided opinions in regard to taking out the cervix in disease of the uterine body. The analogy between the breast and the uterus is striking, both as to disease and as to questions of treatment. If in benign diseases of the breast total extirpation is to be seriously considered, so in benign diseases of the uterus is total hysterectomy to be considered. If in malignant disease of the breast, removal of the breast with the pectorals and the contents of the axilla is essential, so in malignant disease of the uterus it is essential to remove the whole uterus and contiguous healthy tissues with the

affected lymph glands. I have had more cases of amputation of the breast than of removal of the uterus. Of breast amputations the number is very large. In the number of benign breast tumors, especially those reported by Dr. Whitney as chronic mastitis with retention cysts, and operated upon by partial excision, there have been a few in which a cancerous focus was either overlooked at the time of operation, or has developed since. When such a thing happens it seems to me a very serious disaster, especially because in every case the patient, relying on the report of benignancy, returned too late for successful operation, and has miserably perished. In these cases—few in number, it is true—my feelings of self-reproach have been acute. So, in benign tumors of the uterus, would be my feelings of self-condemnation were there many cases of malignant degeneration of the cervix after supravaginal operation for benign growths.

I have had three or four cases in which the breast tumor was regarded by Dr. Whitney as benign, and I have therefore limited the operation to extirpation of the tumor itself, only to later find unmistakable cancer. I reported one or two of these before this Association at the Atlanta (Georgia) meeting, advocating in all doubtful cases total extirpation of the breast.

With reference to removal of the cervix in benign tumors of the uterus, I am open to conviction. The arguments of the reader have appealed to me strongly. In my own practice I do not remove the cervix in benign tumors. Of all my hysterectomies for fibroid, in but one have I found malignancy of the uterine body. In the paper just referred to, I spoke of this case which was watched for a number of years by a skilful and conservative physician, and in which we found not only a fibroid, but also sarcoma of the uterus. The patient died of general recurrence. I can recall but one instance in which cancer of the cervix has developed after supravaginal amputations for fibroids.

With reference to the liability to recurrence, it seems to me that the percentage of cases in which cancer of the cervix has developed after these operations is very small. I have operated upon a number of patients who had cancer in two separate places. I recall three cases of cancer of both breasts, in which the tumors, by size, shape, and duration, were precisely alike. Both breasts were simultaneously affected, and neither growth was dependent upon the other. In the

same period of time I operated upon different forms of malignant disease in breast and uterus in the same patient. But these experiences are so extremely rare in thousands of personal observations of malignant disease at all stages that they go to prove that it is also extremely rare for two radically different neoplasms to grow in the same organ. Cancer as the result of irritation is to be expected, but cancer as the result of an innocent tumor, like a fibroid, is not. I am convinced that the cervix left after supravaginal amputation for uterine fibroid is no more liable to disease than any cervix, and that there is no more reason for removing such a cervix than there is for removing every cervix. I cannot but feel that supravaginal amputation has less danger than total hysterectomy, and that this added risk is unjustifiable except in malignant disease either in the cervix or in the fundus. In fibroids the greatest good to the greatest number will follow the less dangerous course.

Dr. Herman J. Boldt, of New York City.—So far as the occurrence of malignant disease of the cervix after supravaginal hysterectomy is concerned, we must admit that it may take place. I have not personally observed cancer of the cervix following supravaginal hysterectomy for myomata of the uterus, but that it does sometimes occur we must accept; but the mere fact of having left the cervix should be a causative factor in the production of malignant disease, I deny. I do not believe it has anything to do with it.

So far as the relative risks between supravaginal amputation and panhysterectomy are concerned, I do not consider of any importance. One who is accustomed to do pelvic surgery can do one operation nearly as quickly as the other, and with as equally low mortality rate. Probably there is five to ten minutes' difference between the two operations.

The main question before us, from my view point, is, What is the condition of the vaginal vault subsequently? In the majority of instances it makes no difference whether we have a good vaginal vault following supravaginal amputation, or whether we have a vaginal vault with more or less cicatricial tissue in the vault of the vagina. But occasionally it does make a difference. I have seen two or three cases of extreme neurasthenia as the result of scar tissue, and I do not think it desirable to make a panhysterectomy when the cervix is in a fairly normal condition. That is the only

guiding feature for myself, and one which I believe is of importance. When the vaginal portion of the cervix is diseased, we ought to take the cervix out; but otherwise I think it is better to leave it, because with it we can leave a better pelvic floor.

Dr. Robert S. Hill, of Montgomery, Ala.—I cannot bring myself in accord with some of the views that have been expressed. It seems to me there are as good reasons for leaving the cervix in some cases as there are for removing it in other cases. When we consider the anatomy of the pelvic floor we must conclude that the removal of the cervix, notwithstanding the method suggested by Dr. Bovée, will weaken the pelvic diaphragm and thereby make postoperative vaginal hernia more liable to occur. Again, if I remember correctly, the verdict of the majority of the profession is that panhysterectomy is an operation of greater magnitude—with a greater mortality—than supravaginal amputation of the uterus. When we consider the increased traumatism in the former—the increased liability of injuring the rectum, the increased liability of wounding the bladder, the increased chances of interfering with the ureters, either directly or through cicatricial tissue formation, and the increased chances of infection by virtue of entering the vagina (I am not unmindful of the contention of some members of the profession that the vagina in a normal state is practically sterile. This indeed may be true from an obstetrical point of view, but I venture to assert that the surgeon who enters the vagina from the peritoneal cavity, or the peritoneal cavity from the vagina, without first treating the vagina as a septic area, will be judged guilty of an offense against good surgery)—I repeat, when we consider the increased traumatism of panhysterectomy as compared with supravaginal amputation of the uterus, we must conclude that the verdict of the majority of the profession is a just—a righteous verdict. But what have we to say of the assertion of some of the speakers that panhysterectomy in their hands has been as free from danger as supravaginal amputation of the uterus? We do not—we cannot question the honesty of purpose that prompted the investigation of their results, nor do we question the accuracy of their observations. However, on the stage of surgical activity, as in every other department of human endeavor, some men appear to better advantage in one role than in another, and we are

inclined to believe, and to insist, that the results in panhysterectomy achieved by these surgeons simply testify to a superior skill on their part in doing this particular operation; an expertness which the average surgeon may devoutly desire, yet can hardly hope to acquire. Now then, from the very nature of things, the majority of operations must be performed by surgeons of average ability. It therefore follows that in establishing rules and regulations for the preservation of human life we should consider the capacity of the average surgeon to live up to them rather than the capacity of the surgeon of superior skill to do so. In other words, it is a question as to what operation is best in the hands of the average surgeon—the man who, from the very nature of things, must do most of the operating. Considering the matter from this standpoint, it seems to me, supravaginal amputation of the uterus still has a place in the category of legitimate surgical procedures.

As to the effect of amputation of the body of the uterus on appearance of cancer in the cervix, I know of no observation or evidence indicating that it increases the frequency. Unless it does this, then to advocate the removal of cervix for fear of malignant changes would be tantamount to advocating the removal of the cervix of every woman as she approaches the menopause as a preventative of carcinoma. I hardly think the time has come for the profession to assume such an advanced position in preventive surgery.

Dr. Bovée (closing).—I have nothing to say in closing, except to thank the various gentlemen who have discussed my paper. I was in hopes that I would hear some of our laboratory workers have something to say on this subject.

# AN IMPROVED AND ACCURATE METHOD OF LOCATING FOREIGN BODIES WITH THE *X*-RAY.

By Robert Carothers, M.D.,
*Cincinnati, Ohio.*

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The forty years which have elapsed since our Civil War have proved that an individual can be quite comfortable and yet be the receptacle for a foreign body somewhere in his anatomy. There are today many hundred veterans of the Civil War who have carried bullets within their bodies all these years and experienced very little or no real discomfort from the same. This statement is made from an experience as an assistant surgeon for one year in one of the National Military Homes and as a pension examining surgeon for fifteen years.

Granting this statement to be true, a foreign body within the anatomy is not always a menace to good health and it does not become necessary and imperative that all foreign bodies should be removed. While this is undoubtedly true, it is equally true that foreign bodies do, at times, cause trouble and should be removed. Among the causes for removing them are: A suppurative sinus leading from the foreign body; pressure upon some important structure, such as the brain, eye, nerves, vessels, tendons, etc.; obstruction to some inlet or outlet to the body such as the esophagus, trachea, or intestinal tract; and in these days of neuroses it becomes, at times, imperative to remove a foreign body for the relief of neurasthenia, and it can, with regret, be said that it does not always relieve the neurasthenia.

Since the introduction of the *x*-ray this cause is on the increase. An individual who is the recipient of a foreign body does not go very far before someone, either solicited or otherwise, makes a radiogram of the part, which in many instances is known to either excite or increase neurasthenic symptoms, and then the foreign body *must* come out.

It has occurred to me that it was a very wise provision of the Almighty that aseptic surgery should have been so well under way when the *x*-ray came on for its share in advancing science. In the days of old many a dirty instrument or finger caused infinitely more trouble searching for innocent bullets than did the bullets themselves. How much this would have been increased if the *x*-ray had then been at hand, one can only guess.

This would argue, then, that asepsis is of prime importance in foreign body surgery; so also is a surgical experience. It is no work for the novice. I have, myself, seen two failures by young men who, after radiograms had been made, viewed the work from a light-hearted standpoint and in the end were glad to have assistance. Even under the most advantageous circumstances it is not always easy work, leading one not infrequently into very dangerous waters and sometimes trying not a little one's patience and willingness to persevere in a long search. The easy cases in dexterous hands are misleading. That coolness and equipoise a surgeon acquires from experience is very much to be desired in this field of surgery, and, if the field can be made dry with tourniquet or otherwise, it is a decided advantage.

Whatever else may be said, the most important step in foreign body surgery is some definite or fixed idea of the location of the foreign body. The *x*-ray has been of untold value in developing this step. Stereoscopy as applied to radiography, called stereo-skiagraphy, is the most improved method and would seem to give the most accurate location of a foreign body. The object of this paper is really to demonstrate the stereoscope.

This idea has been worked up by many radiographers in different parts of the country independent of each other; therefore, no one can claim unchallenged priority. The instrument which I show you was designed and made by Mr. Kelley, of the Kelley-Koett Manufacturing Co., and to him I am indebted for the mechanical skill in working out crude ideas.

The ordinary *x*-ray plate looks flat. The stereoscope gives a perspective view very much as if the parts (bone and bullet) were before you in nature.

Two separate plates are made of the same part without changing in the least the position of the part skiagraphed, which is placed on the compression diaphragm table, so that the rays may be partially obstructed, and in this way one plate can be removed and a second supplied. The compression diaphragm is discarded and replaced by a hollow double inclined plane or platform at an angle of 160 degrees. With a plumb-bob the apex of the double inclined plane is centred through the centre of the part to be skiagraphed onto the centre of the plate. The plates are made with the tube, eighteen inches above the plate, in the tube holder placed one and three-eighths inches to either side of this central point, so that they are made from points two and three-quarters inches separated from each other,corresponding to the distance between the human eyes.

The developed plates are placed in highly illuminated boxes, facing each other, and are from them reflected into two small looking-glasses, which are placed at a right angle to each other with the apex of the angle placed between the eyes of the observer in the stereoscope. One now looks into the two looking-glasses; each eye in a separate glass sees a separate plate; then by adjusting the plates and the stereoscope to get an exact focus the two plates are made to look as one; and it would appear that there is one looking-glass, behind which the observer looks, reflecting the object back onto the retina, producing a perspective view. This

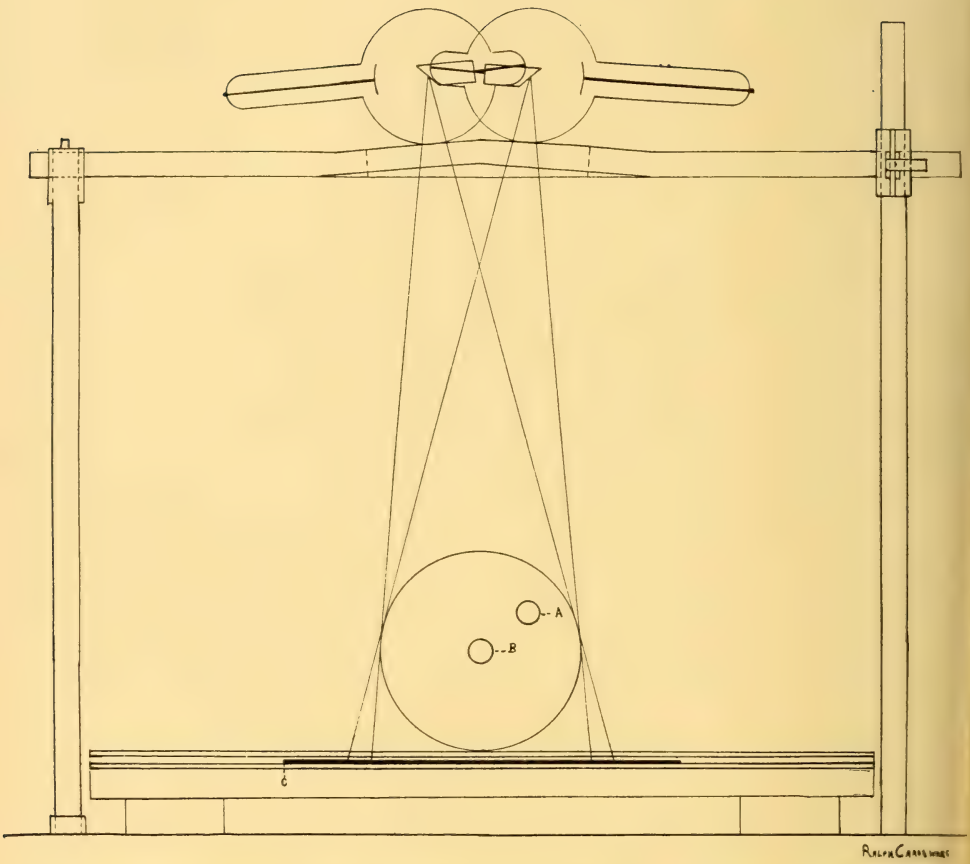


FIG. 1.—Diagram showing method of obtaining stereoskiagram. *A*, bullet or foreign body; *B*, bone; *C*, plate for exposure.

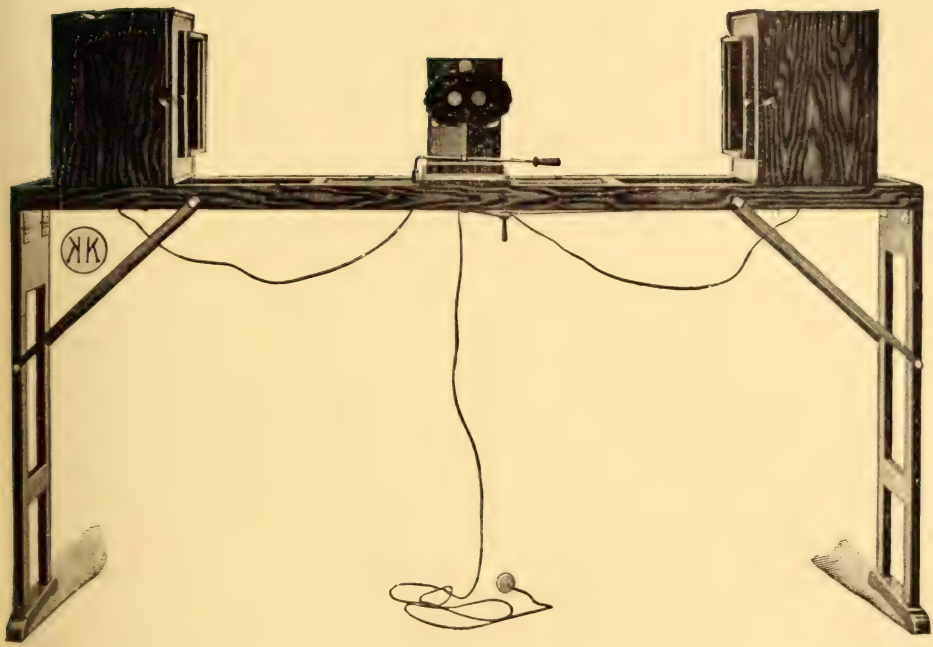

FIG. 2.—Apparatus for viewing the plates so as to obtain stereoscopic effect.

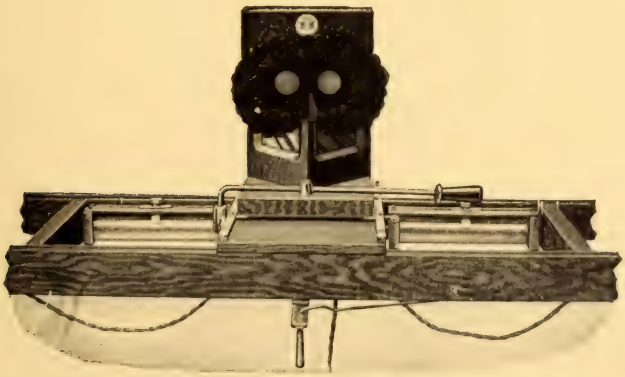

FIG. 3.—The stereoscope shown in Fig. 2, enlarged.

method is of especial value in locating foreign bodies in the spine, thorax, and abdomen, where it is impossible to get a plate which is not flat by any other method.

## DISCUSSION.

Dr. C. E. Caldwell, of Cincinnati.—It has given me great satisfaction to have Dr. Carothers bring this subject before this Association, because the use of the stereograph in the Cincinnati Hospital has been attended with the most gratifying results. Of course, in any city institution, and particularly one lying so far south in this country as Cincinnati, we are apt to have a number of shooting scrapes, and a good many foreign bodies will be introduced with malicious intent by others. The stereograph has been most satisfactory in the locating of these bodies.

To relate one case which demonstrated its utility: About a year ago a man was shot in the left side, and the bullet was shown to be lodged immediately in the spine by the development of symptoms of myelitis, and by means of the stereograph it was demonstrated that the bullet lay in the spinal canal. It was easily demonstrated, as you could imagine, within the spinal canal. The result was I made a laminectomy, and the ball was located within five minutes after the first cut. It was found lying on the dura transversely and completely compressing the cord. The dura was not injured. The ball entered laterally the spinal canal; it was quickly removed, and I congratulated myself that I had achieved a triumphant result in doing the lamentable operation of laminectomy, when the patient, the next day, developed symptoms that were alarming, from which he died. Autopsy showed hemorrhage had taken place into the pleural cavity. No vessels were cut in the dural cavity. The patient died of hemorrhage.

This is one of many instances where the bullet was accurately and definitely located by means of this instrument, and I am particularly glad that Dr. Carothers has brought to your attention so perfect a specimen of this instrument.

# SURGICAL TREATMENT OF GOITRE, BASED UPON THREE HUNDRED PERSONAL OPERATIONS.

By C. H. Mayo, M.D.,
*Rochester, Minn.*

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Until recently, the medical profession of America has been somewhat backward in advocating the surgical treatment of goitre. More especially has this been true regarding that type of goitre known as Graves' or Basedow's disease. The great majority of those operated upon for goitre, other than the exophthalmic form, do so to be relieved of a great nuisance which may be causing tracheal irritation or pressure, impairment of the voice, difficulty in swallowing, or local neuralgias. Often, too, the weight, size, and appearance of the large tumors urge them to operation. There is also the fear, not unfounded, of later malignancy of the growth. Among the 300 operations made, and a large number of cases seen but not operated upon, nearly every disease common to this organ was found except teratoma, tuberculosis, and hydatid. There were 110 operations for exophthalmic goitre, with 9 deaths, 4 of which occurred in the first 16; 3 in the next 30, and 2 in the last 64. In men, only 5 cases of hyperthyroidism were seen. Of malignant diseases, 2 sarcomas and 6 carcinomas were operated upon. Following the removal of carcinoma, there was 1 death on the third day from collapse of the trachea. Of the other 190 cases there was but 1 fatality, and this occurred on the eighth day from pneumonia in a patient who had a large colloid goitre. Two cases suffered from acute hemorrhages into previously existing goitres.

Fig. 1

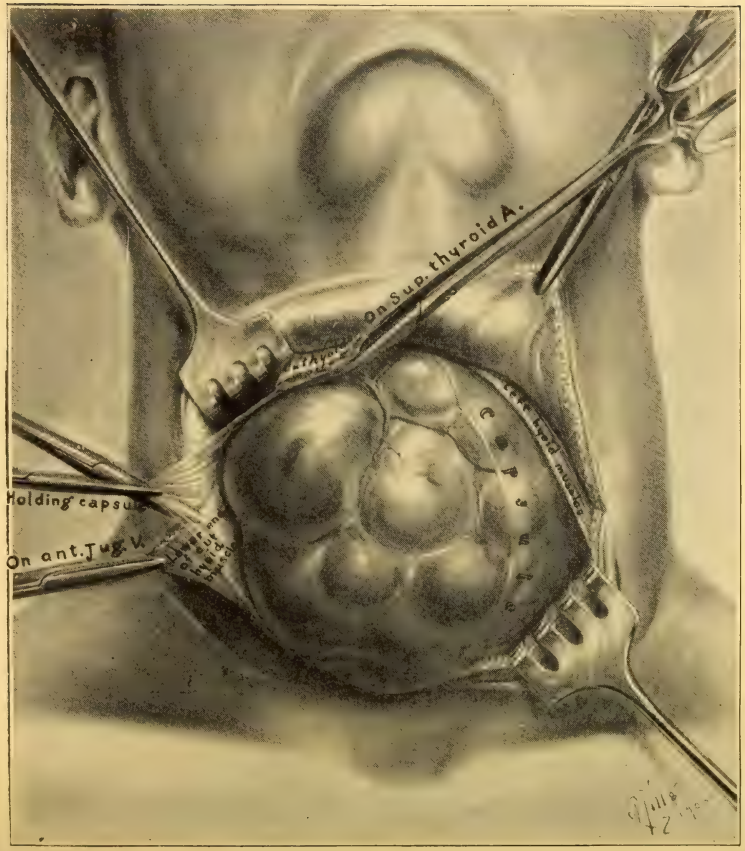
On Sup. thyroid A.
Holding capsule
On ant. Jug. V.

Fig. 2

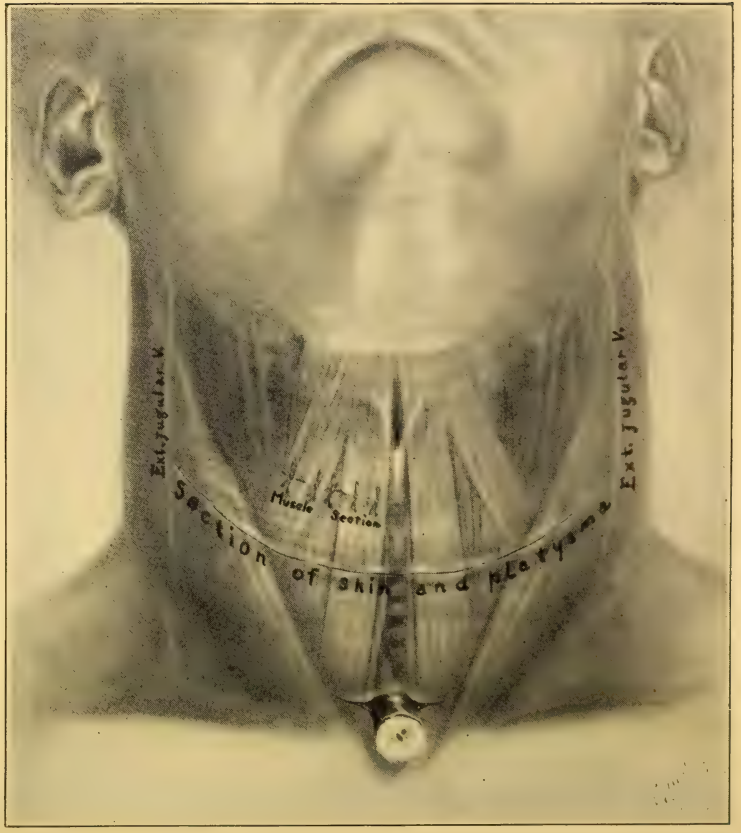
Ext. jugular V.
Ext. jugular V.
Muscle Section
Section of skin and platysma

Fig. 3

Excluding those cases presenting hyperthyroidism, colloids represented 54.5 per cent.; adenoma, 30 per cent., and cysts, 5 per cent.; 4.2 per cent. were malignant, and 2 per cent. were fetal adenoma.

Many colloid and adenomatous goitres were in various stages of transformation. Irritative conditions had developed calcareous deposits in a few colloids of long standing. The glands removed for hyperthyroidism presented hypertrophic changes in cells and loss of colloid substance. In those who had developed Basedow's disease upon pre-existing goitre, the glands showed areas of hypertrophy intermixed with colloid areas.

The simple goitres appearing in girls at puberty and during the early years of menstrual life, are not operated upon unless they change later to other types of tumors.

## PREPARATION OF THE PATIENT.

The condition of the patient has much to do with the outcome of the surgical treatment. The urgency of an operation for goitre is never so great that we should operate upon those who are suffering with a lung complication, as pneumonia causes too high a percentage of the deaths which occur to take such chances. The state of the heart in patients with ordinary goitre is not so serious as in those with hyperthyroidism. Known heart diseases, such as valvular disturbances, rarely cause the sudden deaths, during operation, that are ascribed to myocardial changes.

In exophthalmic goitre, a rapid heart action of 120 to 150 does not constitute an extra hazardous risk if the tension is even and the action steady. As a rule, a few days' observation should be given these cases before recommending operation. Patients who are not advised to have immediate operation are often kept under observation for weeks until we are satisfied of their ability to withstand surgical treatment. Aside from

rest, internal treatment consists of extract of belladonna, $\frac{1}{6}$ grain, with quinine in pill form given three or four times a day. If no improvement is noticed, we use the *x*-ray exposure to the extent of skin irritation. Such cases as have diarrhea or gastric disturbance from the excessive nervous exhaustion and toxic influence give a higher mortality if operated upon at such times. Acute cases, in patients under twenty years of age, require great care and preparation to secure a successful result.

### ANESTHETIC.

The choice of anesthetic in goitre operation is a most important question. Whether it be some kind of local or some special method of general anesthesia, is frequently decided by the prevailing fad of the day. Kocher is often referred to as employing the local anesthesia. It could also be said that he used chloroform in hundreds of cases without a death from the anesthetic, and of his ether cases there was possibly one death from bronchitis. Von Eiselsberg likes a general anesthetic, as does Shepherd, of Montreal. We much prefer it to the local, having found only 13 cases out of over 300 in which we considered infiltration advisable. Operating upon other necessary and serious surgical conditions in those suffering with hyperthyroidism occasions about the same risk as operating upon the gland itself.

The reason given by many for the choice of local anesthesia is that by its use the conscious patient may phonate upon request and this lessens the danger of injury to the recurrent laryngeal nerve. A very forcible reason for its use is the lack of professional anesthetizers, which hampers many surgeons in their work, compelling them to divide their attention between what is, at best, a serious operation and the anesthesia. We employ the open, drop method of anesthesia. Profound anesthesia is unnecessary and should be avoided.

Twenty to thirty minutes preceding the beginning of the

anesthetic, the patient receives a hypodermic of from $\frac{1}{6}$ to $\frac{1}{4}$ grain of morphine and $\frac{1}{120}$ to $\frac{1}{100}$ grain of atrophine. This allays much of the nervousness and prevents the disagreeable bronchial mucus, which comes possibly from irritation of the ether as well as from local traumatism of the trachea, which is so frequently bared a distance of two to four inches in the operation. Much of the respiratory embarrassment at this stage is from traction upon the tumor, which displaces or distorts the trachea.

As to the shock of the anesthesia, we have seen exactly the same type of shock follow the local as the general anesthesia in operating for exophthalmic goitre. In our experience, we can work more rapidly with the patient under general anesthesia, the operations being usually completed in from twenty to forty minutes.

## OPERATION.

During the operation the table is maintained in a slanting posture (reverse Trendelenberg) and a roll of gauze is placed under the patient's neck to facilitate dissection, unless the position embarrasses respiration.

Incision. In our early work upon goitre we employed the angular or bayonet type of incision on the inner side of the sternomastoid muscle. This incision could be made sufficiently large to give a good exposure, but the resulting scar was anything but cosmetic. The greater number have been made through the ordinary collar incision, which closes perfectly with an almost imperceptible scar on the front of the neck, parallel with or in one of the skin lines.

The incision is made from one external jugular to the other, unless the tumor is a small one. The platysma myoides muscle is incised in the same manner, and lifted above and below with the skin flaps. The dissection is sufficient to expose the fascia covering the lower portion of the sternomastoid and the sternohyoid muscles from the thyroid cartilage to the

sternum. The thyroid gland is brought into view by a vertical incision between the anterior jugular veins and the two sternohyoid muscles. These muscles, together at their upper portion, separate above the sternal notch; while the two sternothyroids reverse this form, separating at their upper portion.

Spreading these muscles in the mid-line brings the operator upon the fascial covering of the gland, and exposes many large veins. Small tumors and fairly large cysts can be removed through such an incision without muscle section. Operations upon large tumors and most cases of hyperthyroidism, where extirpation is desired, are made safer by having a muscle section of part of this group of muscles on at least one side. This is made by cutting the muscles between two forceps, which are placed upon them at their upper portion close to the thyroid cartilage. Such a section does not injure the nerve supply which is derived from the communicans hypoglossi, descendens hypoglossi, and ansa hypoglossi. Usually, the sternothyroid will not require section, as its upper portion is attached at a considerable distance from the mid-line. Turning the muscles held in the lower forceps outward, the region of the superior thyroid artery comes into view, and this important vessel with its vein is caught. The lateral vein of the capsule is exposed by muscle traction and cut between clamps; this is also done with the inferior of the same side.

The capsule is incised along the side of the gland, and as the tumor is lifted the capsule is brushed back with gauze, the posterior capsule being left in place. As a number of deaths following thyroidectomy are from secondary hemorrhage, great care should be taken with the application of ligatures. The vessels in this region, including the inferior thyroid, are caught as seen in the dissection. The inferior thyroid is ligated at times before it reaches the capsule of the gland. The isthmus is crushed with large forceps and closed with locking suture. In many of the cases of hyperthyroidism it is deemed best to remove the lower half of the left gland,

as well, the dissection being slanted from the top of the isthmus to a point befow the lateral vein.

In removing the thyroid from the posterior capsule which covers the recurrent laryngeal nerve and parathyroid, great care is exercised in not removing any gland-like bodies for fear that they are parathyroids.

The investigation and experimental work of the past few years tend to show that these small glands perform some most important function and that their removal causes tetany. While the loss of the thyroid in infancy prevents development and produces and induces the condition of cretanism, its loss later may cause degeneracy and in the adult develop myxedema. The removal of one or two parathyroids in the extirpation of the gland in hyperthyroidism may cause sufficient nerve disturbance to add great danger to the operation. Injury of one laryngeal nerve by division is not so serious a matter as was supposed, the ordinary tone of voice being only slightly hoarse, the opposite vocal cord seems to compensate fairly well except in the efforts to speak loud, when the sound may become a mere whisper. Small nodular tumors in the gland, separate follicles of encapsulated colloid, adenomas, and cysts are best treated by enucleation. Nature has outlined the useless and diseased tissue by encapsulating it. The same rule can be observed in their removal as in the treatment of cysts in other regions and organs. Penetration of the gland capsule or part of the gland tissue is made until the unmistakable lustre of the encapsulation is seen. The space is closed with several lock sutures after enucleation. In such a method of operation there is no risk of injury to the parathyroids or nerves, and there is no increased danger from myxedema over their former condition, as the active gland is not removed.

Retrosternal growths, being usually encapsulated, can be safely elevated if the exposure is not ample; increased space can be obtained by splitting the lower skin flap in the centre to the sternum.

Tracheal traction, in elevating the tumor, often causes great temporary difficulty in breathing. As a rule, the softening of the tracheal rings is lateral, and from pressure.

Twice we found it necessary to suture the trachea to lateral tissues to prevent its collapse after removal of the gland. In carcinoma we have found the trachea to be softened by anterior pressure and to be frequently invaded by the disease. Slight anomalies in development of the thyroid may cause unusual tumors. Accessory glands are found from the foramen cecum in the tongue to the isthmus, also laterally beneath the gland. We have seen two thyroids of the tongue and two over the thyrohyoid space. Twice there was not a vestige of gland on one side of the trachea. In another case, a cystic thyroid formed between the esophagus and the trachea, the latter lying just beneath the sternohyoid muscles.

After removal of the gland section in cases of hyperthyroidism the wound area is well washed with Harrington's No. 9 solution: alcohol, 640 parts; hydrochloric acid (chemically pure), 60 parts; water, 300 parts; and bichloride, 0.8 of one part. This acts as a caustic and irritant, causing the wound to be automatically washed with serum, which is not absorbed but is drained through a separate stab sound just above the sternal notch. The majority of cases are drained for a day or two. It increases the comfort of these patients, although in a few instances small tumors are removed without drainage.

If the anterior group of hyoid and thyroid muscles has been severed, they are now united. The platysma myoides muscle is closed by a running suture of catgut from one angle of the incision to the other, the same suture being run back as a subcuticular suture and tied to the first end of the thread. If a subcuticular suture alone is employed, the platysma retracts and later pulls the scar, resulting in the formation of a false keloid.

In all cases a rectal enema of normal salt solution is

given slowly without pressure after operation, and this is repeated at intervals the first day. Especially is this done after operation for hyperthyroidism, the quantity given being from two to three quarts in all. If they develop diarrhea, the saline is given at intervals subcutaneously in a somewhat less quantity.

We expect a quick pulse and some rise in temperature on the second day; from one to four degrees above normal. This is not looked upon as serious and subsides before the third day in ordinary goitres, and by the fourth day in exophthalmic cases. No great effort is made to reduce either. We have secured no permanent benefit from adrenalin. Strychnine is dangerous in quantities sufficient to cause an effect. Should these patients have excessive sweating they are given atropine. If very restless, as well, morphia is also given as needed and the salines pushed. An almost uncountable pulse in those accustomed to such condition is not necessarily dangerous if their mental condition and expression remain good.

In cases of hyperthyroidism, it may be that removal of one side is not sufficient to cause more than a partial recovery. A second operation to remove a portion of the remaining gland is then advised and personal experience indicates that this is fraught with much less risk than the first operation.

There are no more wonderful results in surgery than the immediate effects of operations upon the thyroid. These patients are most grateful, whatever the size or character of the gland removed.

The operative recovery is rapid, it being rare that they are confined to bed more than two or three days and the hospital time does not average more than one week.

All but one of the deaths following operations for hyperthyroidism occurred within twenty hours. In fact, we have but little to fear over the outcome of the operation unless valuable time has been wasted in experimental medication, until heart complications develop unavoidable risks.

## DISCUSSION.

Dr. George W. Crile, of Cleveland.—I was delighted to hear Dr. Mayo's paper. It represents the status of the best authority in surgery today. I want to ask him two questions:

First, whether he has had any experience in the use of Beebe serum? If so, what were the results?

Second, if Dr. Mayo can tell us the group of cases he rejects for operation in hyperthyroidism?

In a series of 114 operations upon the thyroid gland, my smaller experience has compared very well with the conclusions Dr. Mayo has reached, with the exception that I have been unable, thus far, to reach the low mortality that he is able to report from operations in Graves' disease. I have noted a great difference in patients with so-called Graves' disease, as to the variety of the case. If it is a young person who has acute Graves' disease, coming on without previous goitre but enlargement appearing with other symptoms, I have found the operative risk very considerable. On the other hand, in the late cases of colloid goitre of long standing, if the symptoms of Graves' disease appear, I have found these cases comparatively safe risks.

Among the 114 cases operated, I have seen 5 cases of carcinoma and 5 of sarcoma. In 1 of the cases of carcinoma I noted unmistakable symptoms of Graves' disease.

My experience has borne out the conclusions reached by the essayist: that in Graves' disease there is histologically an increased number of true thyroid cells, and that colloid material is diminished. Operations for simple goitre, as Dr. Mayo has said, are safe. In the last 70 operations for this disease there has been no fatality. I have had three deaths in 20 cases of Graves' disease, none of which occurred in the cases of Graves' engrafted upon colloid goitre.

Dr. Randolph Winslow, of Baltimore.—I have been very much interested in the paper read by Dr. Mayo. I had the opportunity of seeing some of this surgical work done abroad this summer at the Kocher clinic and elsewhere.

I have been particularly interested in the manner of the anesthetic employed in these cases. Of late I have been using scopolamin-morphin injections as a species of general

anesthesia, and the infiltration of the skin with the Schleich solution for strictly local anesthesia, and I find it works well. The patients, as a rule, do not complain, and even where they have complained a little, in asking them subsequently how much they suffered, they will say scarcely any; so that the advantage, as has been stated of this local anesthesia, is that the patient is enabled to give some indication as to whether injury is being inflicted or not upon the recurrent laryngeal nerve.

I understood Dr. Mayo to say that the patients were placed in the partially upright position. One can see the great advantage of a marked upright position, in that it aids the venous blood to flow from the neck and enables one to make a clean dissection much more feasibly than if the patient is in a low position.

What little surgical work I have done in these cases has been quite satisfactory with the methods of anesthesia I have mentioned.

Dr. Mayo (closing).—Operations on ordinary goitres are so safe now that I hardly expected any discussion in that line.

We look upon Graves' disease as being of three types, namely: vascular goitre, true hypertrophic goitre, and the cases engrafted on an old pre-existing goitre. Some of the latter are the worst possible cases to exhibit all the types and all the symptoms of Graves' disease. Vascular goitre is more rapid in its growth, and I will state that Kocher has operated on ten of those cases without a death. I have not had a death in that type of goitre. Of those I have seen there have been more cases in men than in women, considering the small proportion of men, as compared with women, who have exophthalmic goitre. Like Dr. Crile, I find goitre develops rapidly in young girls, high-school girls, seventeen or eighteen years of age, and when exophthalmic goitre develops rapidly operation is a serious task, and the one thing that has led me to operate on such cases, after observation for a few days, is the lack of fluctuation in the pulse. If I can count the pulse for fifteen or twenty seconds and not have two or three beats get away from me, I do not hesitate to operate; but if such a condition occurs I put the patient off until I can count the pulse; I do not care if it is 172. I had one patient who, after three months' observation and treatment with *x*-rays, never had a pulse of less than 172. In addition to the tension

if the pulse of a patient should run about thirty beats quickly when she rises from a chair, or if you speak to her quickly and the pulse jumps up, it is an unfavorable risk, and you should watch that patient for some time before operating. I lost 4 patients out of the first 16; 3 in the next 30 cases, and now I have had 300. I have had over 70 cases with 2 deaths. So there is something to be learned in operating on these cases. It is said that a certain eminent oculist, who operated for cataract years ago, spoiled a hatful of eyes before he learned to take out a cataract right.

With reference to the question asked by Dr. Crile concerning the serum, I have sent 40 of these exophthalmic goitres to Dr. Rogers and Dr. Beebe for the development of their serum. The best serum is made from this type of gland without colloid. In the true hypertrophic form of goitre in which we find three or four layers of cells lining the acini, with unfolding of these cells like the cortex of the brain, there is almost no colloid in the centre as in the normal gland. This type of gland gives them a protonucleoid; and they have in their report of some 98 odd cases, 55 with 4 deaths.

I recently had a case brought to me on a bed. The patient was a man, and if I had operated on him in the extreme condition he was in, he would have died. He was going to die anyway. He was given the serum which Dr. Rogers sent me, and lived twenty-four hours after it was given. This was a case in which there was no gland to be felt in the man's neck; yet, he had every symptom of exophthalmic goitre—diarrhea, stomach disturbance, vomiting, protruding eyes, tachycardia, nervous symptoms, and fine tremor. At the autopsy I got a gland that weighed three ounces. The normal is one to one and a half ounces. Of 40 cases reported by Thompson, in 28 there was enlargement of the gland, while 12 did not have an enlarged gland. So it is classed as an intestinal toxemia.

So far as the anesthetic is concerned, I must say that I like to have these patients asleep, especially in the presence of hyperthyroidism. A slow operation on a patient suffering from hyperthyroidism is dangerous, from the fact that the patient is weak. I have had death take place ten hours after operation where I used cocaine and worked more slowly, and I cannot say that local anesthesia was particularly advantageous over a general anesthetic.

# TOXEMIA OF PREGNANCY.

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The pernicious vomiting of pregnancy, acute yellow atrophy of the liver, and eclampsia, are generally grouped together as manifestations of the toxemia of pregnancy. While eclampsia is admittedly a toxemia, it seems unwise to classify it with the other two, from which it differs widely in symptoms, pathology, and prognosis, if not in other respects. For this reason it seems to me that eclampsia should be studied entirely apart from the other two types. I wish it to be understood, therefore, that wherever the term toxemia is used in the present article, it refers only to the toxemia responsible for pernicious vomiting and acute yellow atrophy, and not to eclampsia unless so stated.

The most pronounced lesion to be found postmortem in a case of toxemia of pregnancy, is a degeneration and necrosis of the parenchyma of the liver. While this lesion is characteristic of the toxemia of pregnancy, it is not peculiar to it, since practically the same changes may occur in toxemias from other causes, the difference being in degree rather than in kind. For instance, an extreme degree of hepatic necrosis is brought about by phosphorus poisoning. Similar, though less pronounced changes may occur after chloroform anesthesia, as pointed out by Bevan and Favill[1] and demonstrated experimentally by Offergeld.[2] Muller's[3] experiments go to prove that similar effects may follow the administration of ether and other inhalation anesthetics, though the tend-

ency is much less marked than is the case with chloroform. Acute alcohol poisoning has been known to produce fatal necrosis of the liver, as noted by Quincke.[4] These are examples of hepatic necrosis caused by chemical substances. As an instance of a similar process due to a vegetable poison, Quincke mentions the occasional occurrence of liver necrosis as a result of mushroom poisoning. The liver changes of yellow fever consist mainly of degeneration and necrosis, not essentially different from that caused by the poisons mentioned above.

The experiments of Traube, quoted by Lusk,[5] would appear to prove that necrosis of the parenchyma of the liver may be brought about by an excess of bile acids in the blood. This information must be accepted second-hand, if at all, since the reference to Traube's article is not given by Lusk. Traube's experiments might well be repeated with a view to confirming his results. If his observation is correct, it throws light on some of the obscure deaths in coma following operations for common-duct obstruction. It would also appear to offer a probable explanation of the pathogenesis of acute yellow atrophy of the liver occurring in males and non-pregnant females. It would be reasonable to suppose, for instance, that ordinary catarrhal jaundice, when occurring in an individual whose liver is excessively intolerant of the presence of bile acids in the blood, may induce necrosis of such a liver in the same way as the various poisons already mentioned. This would be an example of hepatic necrosis due to the absorption and subsequent poisonous effect of a physiological secretion.

It will thus be seen that necrosis of the liver parenchyma may be caused by a variety of toxic substances, besides those peculiar to pregnancy.

The source and nature of the toxins which give rise to the fatal liver changes in pregnancy has been the subject of much theorizing, but so far without definite results. The gastro-intestinal tract, the liver, the kidney, the ovary, and the

ovum[6] have each been designated as the source of the toxins, but this entire aspect of the subject is still in so much doubt that it is hardly worth while to go into a discussion of any particular theory. It is sufficient to say that, whatever its source, it is generally conceded that there is a toxin in the blood which is responsible for the morbid process in the liver.

While little or nothing is known of the source or nature of the toxins which cause the hepatic lesions, something more definite is known of the nature of the lesions themselves. Salkowski's classic experiments on autolysis of the liver were followed by Liefmann's[7] observation of the similarity of the liver changes in fatal cases of toxemia to those of the excised liver undergoing self-digestion. Furthermore, some of the products arising during the self-digestion of the organ, such as leucin and tyrosin, are also present in the blood and urine in many of the fatal cases of toxemia. There is, therefore, good reason to believe that the liver-changes in toxemia are in reality due to a process of autolysis. It is claimed by Wells[8] that when the upbuilding function of a living cell is abolished by loss of its nutritive supply, or impaired by the action of poisons, the last function to be destroyed is the property of self-digestion. In Salkowski's experiments, anabolism was necessarily abolished, owing to absence of cell nutrition, leaving the katalytic process to go on unhindered. It follows that if the higher vital process of anabolism is impaired or abolished by a poison, the cell may attack and destroy itself by autolytic action. The toxins of pregnancy probably induce the liver changes in this way, through the abolition or impairment of the upbuilding function of the liver cells.

Granted that the blood during pregnancy is increasedly toxic, as claimed by Bouchard,[9] we must then assume an insufficiency of the liver function as an essential factor in the production of the morbid symptoms and lesions of the toxemia of pregnancy. The formula for its production would be: insufficient liver and toxemia on the one hand,

resulting in failure of the regenerative function, and autolysis on the other. The substances in the blood, noxious to the liver cells, would first merely impair the hepatic function, thereby leading to accumulation of the toxins peculiar to pregnancy as well as those arising from ordinary metabolism, which the liver in its crippled condition is no longer able to neutralize. When the toxemia becomes so severe as to be intolerable, abortion or premature labor may occur through the effort of nature to relieve the condition by removal of the cause, or, perhaps, as a direct effect of the toxins. This generally occurs before the development of actual necrosis in the liver. The wide destruction of liver cells, as indicated by clinical symptoms, does not ordinarily occur until after the lapse of a period varying from a few hours to a few days after the uterus is emptied. In other words, the removal of the primary cause of the toxemia by terminating the pregnancy, appears to have the effect of aggravating the condition, paradoxical as this may seem. This was true in three of my four fatal cases, the other being practically moribund from exhaustion at the time of delivery. It is also a noticeable feature of many of the reported cases in the literature. This peculiar behavior of toxemia cases is especially noticeable in those which begin as catarrhal jaundice during local epidemics of this disease. These cases do not appear to be seriously sick until after delivery, when the symptoms of acute yellow atrophy supervene and lead rapidly to a fatal issue.

This tendency of toxemia cases to get worse after removal of the primary cause is a matter of supreme clinical importance, and it is surprising that it has not attracted more attention from obstetricians. So far as I can learn from the current literature on the subject, there seems to have been little or no attempt to ascertain its cause or to forestall its effect. Recognition of this peculiarity of toxemia cases forces the conclusion that a new supply of toxins must enter the circulation after the uterus has been emptied. It seems

to me that the source of these new toxins is not far to seek. The postpartum uterus loses weight rapidly after being emptied of its contents, the loss during the first week approximating 50 per cent., according to Williams'[10] figures, the freshly delivered uterus weighing about one thousand grams, and only five hundred grams one week later. This rapid atrophic process undoubtedly results in large quantities of waste products being thrown into the circulation. The burden of neutralizing these products, which must be toxic inasmuch as they are excrementitious, falls upon the liver, which in its already crippled condition is unequal to the task. The impending necrosis of liver cells then becomes actually established.

Of interest in this connection is the fact that there is reason to believe that the process of involution of the uterus is itself a physiological autolysis of the organ, rather than a simple atrophy or fatty degeneration, as heretofore taught. This idea is borne out by the occurrence of peptonuria during the puerperium. Lavene[11] refers to it in his article on autolysis, and something like the same idea must have been in the mind of Fischel[12] before the advent of the doctrine of autolysis, when he advanced the theory that involution was due to the conversion of the hypertrophied uterine muscle into peptone, and its excretion in the urine. Peptonuria presupposes peptonemia, and, as is well known, peptones and albumoses are toxic when present in the blood. It is not to be inferred, however, that these are the only toxic substances resulting from the retrograde process in the uterus. I presume it will be understood that, by the terms peptonuria and peptonemia, the presence of albumose in the urine and blood is meant instead of true peptone, which is never present in either fluid unless artificially introduced.

Granted that the toxins resulting from the retrograde change in the postpartum uterus are apt to be the final influence in determining the development of an impending necrosis in the liver, the importance of forestalling their

effect is obvious. This can only be done by terminating the pregnancy at an earlier date than would otherwise seem necessary. In other words, it is not enough merely to determine whether the liver can withstand the pregnancy toxemia, but allowance must be made for the additional postpartum dose of toxins.

Another element which is no doubt sometimes the determining factor in an unfavorable case is the anesthetic. While this applies especially to chloroform, it must not be forgotten that in order to produce complete anesthesia a toxic dose of an anesthetic is necessary, and, as heretofore stated, all of the inhalation anesthetics are to some extent liver poisons.

Pathology. The liver is the principal seat of the pathological changes induced by the toxemia of pregnancy. The process consists essentially of a degeneration and necrosis of the cells of the liver lobules. Any of the three zones of the lobule described by Virchow may be affected by the necrotic process, but a careful analysis of published reports would indicate that it attacks, by preference, the central and mid-zonal areas. Schmorl[13] believes that necrosis beginning in the peripheral zone is typical of eclampsia, and his views are confirmed by Williams.[14] Both observers, however, attribute the necrotic process in eclampsia to secondary changes dependent upon thrombosis of the vessels in the interlobular spaces. This explanation of the hepatic necrosis in eclampsia serves to support the view advanced by Williams that eclampsia is due to a different kind of toxemia from that giving rise to pernicious vomiting and acute yellow atrophy. According to this explanation, the variety of toxemia which gives rise to eclampsia produces thrombosis primarily, the necrotic changes being a secondary effect, while the toxemia which results in pernicious vomiting and acute yellow atrophy is the direct cause of necrosis of the liver cells. Ewing[15] considers that eclampsia, acute yellow atrophy, and pernicious vomiting are simply different manifestations of the same toxemia. While this question

remains to be definitely settled, it is my personal belief that the diversity of the two types of toxemia will ultimately be acknowledged. In addition to the well-recognized differences in symptoms and pathology, between acute yellow atrophy and vomiting on the one hand and eclampsia on the other, there is a striking difference in their behavior after the uterus is emptied. Barring exceptional cases, the tendency of eclampsia is to get better a few hours after delivery, while that of the other variety is unmistakably in the opposite direction. Furthermore, they differ in prognosis, uncomplicated eclampsia being a much less fatal disease than acute yellow atrophy or pernicious vomiting. It is not unlikely that mixed types of toxemia may occur, giving a clinical picture that may be interpreted as eclampsia or acute yellow atrophy, according to the bias of the individual observer. The existence of these mixed types may be the cause of much of the unfortunate confusion in the terminology of this subject.

In addition to the lesions in the liver, degenerative changes may be present in the kidneys and heart, and in eclampsia small thromboses in the brain and lungs.

Symptoms. Vomiting is a prominent symptom of toxemia. When occurring in the early months this symptom dominates the picture to such an extent that the condition is classed as a clinical type under the name of pernicious vomiting of pregnancy. This early vomiting is apt to be confused with the benign type due to reflex or neurotic influences, but in the later months its significance is not so apt to be overlooked.

Headache is a common symptom, as are also neuralgic pains in other parts of the body. Epigastric pain, probably due to the liver changes, is especially significant.

Increased secretion of saliva is an important symptom present in many of the bad cases. It is most noticeable at night during sleep, as the patient on waking finds a spot on the pillow due to the dribbling of saliva from the mouth.

Air hunger, described by Kussmaul,[16] is a fairly constant symptom. It was a prominent feature in 3 of my 4 fatal cases, and in 4 of the 5 cases seen with others.

Extreme mental depression is present in many cases. The patient takes a most gloomy view of her condition, and frequently expresses a conviction that she will not recover.

Edema of the extremities or face is frequently noted, although it is by no means a constant feature.

Albumin may or may not be present in the urine. Evidences of disturbed metabolism, however, may be quite constantly found.

All of these symptoms precede the development of the necrotic process in the liver, and are due to the toxemia itself. Those which depend upon the destruction of liver cells are coma and stupor, black vomit, bile in the urine, possibly icterus, rapid breathing, convulsions (always in eclampsia, occasionally in acute yellow atrophy), failure of renal function, failure of circulation. Death is the rule after the advent of these grave symptoms, though regeneration of the damaged liver cells, and recovery, are possible.

Diagnosis. Valuable work has been done by Williams, Ewing, and others, towards establishing laboratory aids to the diagnosis of the toxemia of pregnancy. Ewing first called attention to the diagnostic significance of unoxidized proteid derivatives in the urine, while Williams was the first to point out the special significance of an increased percentage of ammonia. The normal ammonia percentage varies from 3 to 5 per cent., and Williams considers it a danger signal when it reaches 10 per cent. or more. I am in no position to express an opinion as to the practical clinical value of this test, having had no experience with it. There can be no doubt, however, that the test is based upon logical grounds. It is well known by physiologists that when, in an animal, a part of the portal blood is diverted from the liver and carried directly into the general circulation by means of an Eck fistula between the portal vein and

vena cava, a considerable part of the urea nitrogen appears in the urine as ammonia.[17] Almost all of the urea is represented by ammonia if the liver is entirely shunted out of the circulation by ligature of the hepatic artery and of the portal vein on the liver side of the fistula. In the light of this experimental data it is to be expected that hepatic insufficiency in the human subject would result in a similar disturbance of the ratio between the urea and ammonia output. While there can be no doubt that the presence of an increased percentage of ammonia in the urine is an indication of a dangerous hepatic insufficiency, it is denied by Ewing that its absence is necessarily of favorable significance. It is to be regretted that the estimation of unoxidized proteid derivatives, and especially the ammonia coefficient, is so complex a chemical procedure as to be available only to those who have access to the services of a skilled chemist. The diagnosis must therefore continue for the present to rest almost entirely on the clinical symptoms, at least so far as the general practitioner is concerned.

Treatment. Palliative measures, to be effective, must be directed not only against the toxemia, but quite as much against the hepatic insufficiency. No method of treatment hitherto employed has appeared to favorably influence the impaired antitoxic-function of the liver. Eliminative and dietetic treatment, intended to reduce the toxemia, while sometimes successful, too often merely postpones the crisis, the morbid condition reappearing at a later date in aggravated form. I have seen several cases behave in this way, and many other examples may be found in the literature. In view, therefore, of the impracticability of treating the hepatic insufficiency and the unreliability of treatment directed against the toxic condition itself, it seems to me that palliative measures should have no place in the management of a recognized case of toxemia of pregnancy. Radicalism is, then, the only alternative, and in the interest of the patient it should be resorted to early and without reference to the

interest of the fetus. Assuming that the diagnosis is established, the indication to empty the uterus is just as urgent as the indication to operate in a case of recognized ectopic pregnancy before rupture. If the condition arises during the early months, the question as to whether to empty the uterus at one sitting, or whether abortion is to be induced, must be decided according to the urgency of the case. On account of the possibly unfavorable influence of anesthesia, non-operative induction of abortion should be given preference, other things being equal. This rule should also be followed in the later months for the same reasons. A protracted labor should not be permitted, however, as the excessive muscular exertion would add its quota to the toxemia already existing. If the case is very urgent, rapid delivery may be accomplished by means of whatever obstetric operation seems to be best suited to the case. Cesarean section may even be performed, by either the vaginal or abdominal method, provided that the exigencies of the case appear to call for such an operation. If, however, for any reason, it is seen fit to perform the abdominal operation, the question arises as to whether it would not be advisable to remove the uterus at the same time, in order to avoid a probable increase in the toxemia as a result of the process of involution.

In any operative procedure on a toxemia patient, inhalation anesthesia should not be employed if it is possible to avoid it, for reasons already stated. Spinal anesthesia, free as it is from the disadvantages of general anesthesia in this condition, is worthy of favorable consideration. In any event, chloroform should never be used in a case of toxemia of pregnancy.

### REPORT OF CASES.

Case I.—Mrs. A., aged nineteen years, primipara, seen with Dr. Wyatt Heflin. Pregnancy uneventful until the beginning of the eighth month, when she contracted what

appeared to be an attack of ordinary catarrhal jaundice, which was prevalent in the neighborhood at the time. The jaundice continued for two weeks, when labor came on, January 14, 1903, about two weeks before full term; spontaneous delivery in four hours. Patient's condition apparently normal except for the jaundice until forty-eight hours after delivery, when she gradually sank into a stupor and died, January 17, 1903, seventy-two hours after delivery. A few hours before death she vomited a quantity of coffee-ground material. A specimen of urine obtained just before death contained leucin crystals, and this finding, together with the clinical picture, lead to a diagnosis of acute yellow atrophy of the liver. The case was reported as such before the Jefferson County Medical Society, February 9, 1903. No postmortem was allowed.

Case II.—Mrs. S., primipara. Only abnormal feature of pregnancy was edema of extremities and face. Urine examined repeatedly for albumin and casts, but always with negative results. Labor began about full term, September 9, 1904. After twenty-four hours, delivery was accomplished by version. Chloroform anesthesia, somewhat prolonged on account of repair of perineal laceration. Patient's condition good, except for pain in right side of back, until the morning of September 13, sixty hours after delivery, when she was found by the nurse in partial stupor. This steadily deepened and jaundice appeared a few hours later and rapidly increased. Death occurred September 14, eighty-four hours after delivery. No postmortem allowed.

Case III.—Mrs. C., aged thirty-four years, primipara. Nausea and vomiting, together with albuminuria, appeared about the end of third month. Condition improved remarkably on milk diet and eliminatives. The symptoms reappeared, together with edema, rather suddenly about the end of the seventh month. After a few days of palliative treatment labor was induced, and after a tedious labor delivery was accomplished by forceps under chloroform anes-

thesia. Black vomit appeared during the anesthesia, and consciousness never fully returned. Scanty urine containing albumin, casts, and bile pigment. Death thirty-six hours after delivery, October 22, 1904.

Case IV.—Mrs. A., second labor. She was attended by me in her first labor, May 16, 1901. She suffered very much from nausea and vomiting during her first pregnancy; difficult forceps delivery. Twenty-four hours after delivery a mild acute mania developed which lasted about one week. Convalescence was slow, but finally resulted in complete recovery. This patient was strongly advised against a future pregnancy, and no further pregnancy occurred until August or September, 1903. On this second occasion she almost died from pernicious vomiting, but steadfastly refused to have the pregnancy terminated until she was almost moribund. Abortion was induced about the second month without anesthesia, and she made a satisfactory recovery. She became pregnant again five months later, and got through the early months very well. At the beginning of the sixth month, however, persistent vomiting set in accompanied by scanty urine and albuminuria. She absolutely refused to consent to induction of labor until her breathing began to be labored, and she had already sunk into a partial coma before preparations for the delivery had been completed. The delivery was accomplished by version, December 6, 1904, the necessary dilatation of the cervix being obtained by means of the Bossi dilator. The entire procedure was completed in thirty-five minutes without damage to the maternal structures. Her pulse was almost imperceptible at the time of operation, and she never reacted, death occurring four hours later.

Case V.—Mrs. O., third pregnancy. Headache, mental depression, edema of face and extremities, increasing albuminuria, and profuse salivation. No improvement under palliative treatment. Labor induced July 22, 1906, at seventh month; living child; uneventful recovery.

Case VI.—Mrs. H., seen with Dr. R. B. Harkness. Third pregnancy. Eclamptic convulsions in first labor; second pregnancy and labor uneventful. One convulsion at fourth month of present pregnancy; improvement under palliative treatment. Easy labor at full term, September 12, 1906. Rapid pulse and labored breathing, with partial stupor, developed a few hours after labor. Condition very grave, but final recovery occurred after a slow convalescence, complicated by endocarditis and thrombosis of veins of left arm.

Case VII.—Mrs. T., seen with Dr. D. F. Talley. Principal symptom, "air hunger" and vomiting of coffee-ground material; pulse rapid and weak; premature labor induced by Dr. Talley, September 12, 1906. Result, recovery.

Case VIII.—Mrs. T., seen with Dr. D. F. Talley. Persistent vomiting of coffee-ground material which gave the reaction for blood with guaiac test; mental torpor and profuse salivation; premature labor induced by Dr. Talley. Result, recovery.

Case IX.—Mrs. B., seen with Dr. E. S. Casey. Spontaneous abortion on September 27, 1906, in third month of pregnancy; uterus curetted on account of retained secundines, October 4, 1906, six days after the abortion. Temperature continued high, and on following day patient developed coma and jaundice. The jaundice deepened rapidly, coma became profound, and death occurred on October 6, eight days after the abortion. The curettage was done under ether anesthesia. No postmortem was allowed.

Bibliography.

1. Bevan and Favill. Acid Intoxication and Late Poisonous Effects of Anesthetics. Jour. Amer. Med. Assoc., September 2–9, 1906.
2. Offergeld. Arch. f. klin. Chir., 1905, vol. lxxv.
3. Müller, B. Arch. f. klin. Chir., 1905, vol. lxxv.
4. Quincke. Nothnagel's Encyclopedia of Practical Medicine.
5. Lusk, W. T. Science and Art of Midwifery, 1893.

6. Edgar, J. Clifton. Clinical Manifestations of the Toxemia of Pregnancy. New York Med. Jour., May 5–12, 1906.

7. Liefmann. Centralb. f. allgemein Path., Band xvi, No. 12.

8. Wells, H. Gideon. Delayed Chloroform Poisoning and Allied Conditions. Jour. Amer. Med. Assoc., February 3, 1906.

9. Bouchard. Autointoxication in Disease.

10. Williams, J. Whitridge. Obstetrics, New York, 1903.

11. Lavene, P. A. Autolysis. Jour. Amer. Med. Assoc., March 17–24, 1906.

12. Fischel. Arch. f. Gyn., 1884, vol. xxiv.

13. Schmorl. Untersuchungen über puerperal-eklampsie.

14. Williams, J. W. Pernicious Vomiting of Pregnancy. Trans. Amer. Gyn. Soc.

15. Ewing, James. The Pathology and Pathogenesis of the Toxemia of Pregnancy.

16. Kussmaul. Quoted by Bevan and Favill.

17. Schäfer, E. A. Text-book of Physiology.

# REPORT OF CASES OF TOXEMIA OF PREGNANCY.

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Case I.—Was seen with Dr. McAdory, March 24, 1906. Mrs. B., aged twenty-four years, three months pregnant; she had suffered great pain in abdomen and has been very much nauseated for about two weeks; during the past week nothing had been retained on the stomach and she was confined to bed. There was no swelling and no albumen in urine, but she was growing weaker every day. The uterus was emptied and vomiting stopped in twenty-four hours; she continued to improve and was well in a week or ten days. During a pregnancy eight months prior to this she had the same symptoms of toxemia and the uterus was emptied at a two and a half months' pregnancy.

Case II.—Was seen with Dr. McAdory, July 8, 1906. Mrs. H., aged thirty-two years, seven months pregnant, this being her fourth pregnancy. At five months she was threatened with miscarriage; this was stopped with morphia and rest in bed. At six months her feet began to swell and she suffered with severe headache; these cleared up under the influence of eliminative treatment; at the seventh month the swelling and headaches returned and were worse than before. In addition to these she suffered extreme nausea and was unable to retain anything; she complained that her sight was defective and that at times she was perfectly blind. She was taken to the infirmary and labor induced by rupturing the membranes and packing the lower segment of the uterus with gauze. Morphine and hyoscine were given

to procure rest and render labor less painful; child was born alive, but could not be resuscitated and lived only a few minutes. In twenty-four hours after delivery all the symptoms of toxemia became much aggravated, her pulse ran high, headache was excruciating, and she became totally blind for a time; under heroic eliminative treatment these symptoms cleared up and she began to improve; vomiting stopped in a few days and she made a good recovery. No albumen was found in her urine until after delivery and then only a slight trace.

Case III.—Was first seen September 13, 1906, aged thirty-eight years; four children, this being her fifth pregnancy; she was within eight days of her expected confinement, being practically full term. When I saw her she had been vomiting incessantly for three days and was very much exhausted; she presented the appearance of an extremely sick woman; suffered pain in region of stomach which radiated through to back; her vomit was just beginning to have the appearance of coffee grounds. She was taken to the infirmary next morning; some rest and sleep had been procured during the night by the hypodermic administration of morphine and hyoscine. Her nausea continued and the vomit became darker; her pulse ran up to 130 per minute and temperature 99.5°. Dr. Jordan was called in consultation and the induction of labor decided on as the treatment. The membranes were ruptured and lower segment of uterus packed with gauze; morphine, and hyoscine were given to procure rest and make labor easy and void of shock. As soon as the os was completely dilated she was placed on the operating table and delivered with forceps; as the pains were not sufficient to insure an early delivery, about one drachm of chloroform was administered during this procedure. After prolonged efforts the child was resuscitated but lived only a few hours. Vomiting stopped in twenty-four hours after delivery, and the patient was well in two weeks. The urine in this case showed nothing.

Case IV.—This case was first seen September 24, 1906, eight months pregnant; had been vomiting for about one week, but was not alarmed as she had vomited late in nine previous pregnancies. When I saw her the vomit was becoming dark; her skin was rather yellow, and pulse 120. Morphine and hyoscine were given to stop vomiting and procure sleep; rectal nourishment and strychnine were also given; she vomited continually except while under the influence of morphine and hyoscine; she suffered great pain in the stomach which radiated to the back. I advised the induction of labor which she vigorously opposed, saying, that she preferred to die rather than have that done.

Her symptoms grew more alarming and she grew weaker, so by the fifth day she consented to have labor induced. I learn since her recovery that at that time her mind had become so affected that she knew very little and remembers practically nothing of what happened. She was taken to the infirmary and labor induced by rupturing the membranes and packing the lower part of the uterus with gauze; morphine and hyoscine were given to induce sleep and give rest; she slept for six hours and awoke with sharp expulsive pains which delivered the child in thirty minutes. It was all over so quickly that I did not have time to reach her before the child was born. In twenty hours after delivery, her temperature rose to a 102.5° and she continued to have an irregular temperature for four weeks; she vomited blood occasionally for a week after delivery, and suffered extreme ptyalism for four weeks. The child was stillborn and from appearances had been dead for several days. She was about six weeks in regaining enough strength to be out and about. The urine in this case showed nothing abnormal by the ordinary tests.

Case V.—Case was first seen September 28, 1906. Mrs. G., aged thirty-nine years; three children; two and a half months pregnant; had been vomiting for more than a week and had been confined to bed four or five days; suffered pain in stomach; skin was yellow and pulse 140. This

patient was carried to the infirmary and the uterus emptied; her nausea ceased at once and she was up in a week; her urine showed nothing of importance.

Case VI.—This case was seen in consultation on October 11, 1906. Mrs. L., aged twenty-eight years; two children; nine months pregnant; normal labor had set in when I saw her; she had been vomiting five days, vomit had become coffee-grounds in color; she had pain in region of stomach; she had suffered with ptyalism for some weeks previous and her pulse was weak and fast. As normal labor had begun I advised the administration of morphine and hyoscine to check vomiting and make labor easy; she was delivered in about eight hours of a living child which was resuscitated with difficulty. Vomiting kept up for several days after delivery; there was also shortness of breath and ptyalism. She made a good recovery. Eighteen months previous to this she began vomiting ten days before full term and had all the symptoms of toxemia; at this time she had coffee-grounds vomit, shortness of breath, ptyalism, etc., and was delivered of a dead child. The urine in this case showed nothing abnormal.

Case VII.—This case was seen November 1, 1906. Mrs. S., aged thirty years, six children; seven and a half months' pregnancy; became nauseated two weeks ago; this nausea increased in severity and one week ago she became so weak she had to go to bed; at this time she could retain nothing and ptyalism was very annoying. During the last week she had suffered with shortness of breath and pain in stomach extending back under left shoulder-blade; she had a blurred and double vision lasting from four days before to three days after delivery; black vomit commenced six days before delivery and lasted for twenty-four hours after the child was born. Labor was induced in this case by rupturing the membranes and packing the lower segment of uterus with gauze; the vagina was excoriated from the uterine discharge so the whole vagina was painted with tincture of iodine

before any intrauterine manipulation was performed; after this her temperature rose to 102.5°; pulse 140; and I feared that she had been infected from the vagina; when pains started she was given morphine and hyoscine which eased her pains and allowed her to sleep until the expulsive pains began; she had only two or three of the latter until the child was born; this child was stillborn and from appearances must have been dead for several days. She began to improve in a few hours after delivery and was up in ten days. Three years previous she developed toxemia at seven and a half months and was unconscious for several days; labor was induced and she made a slow recovery. The urine showed nothing abnormal.

Case VIII.—This case was seen November 5, 1906, aged twenty years, married four months, three months' pregnancy; nausea commenced four weeks before I saw her, this gradually grew worse and she took her bed one week before my visit; she had pain in stomach, shortness of breath, ptyalism, blood vomit, slight temperature, and a pulse of 130. She was anesthetized and the uterus emptied; vomited bright-red blood immediately after operation. This case was in the upper part of the State, 150 miles distant from Birmingham so I did not see her again. I am informed by Dr. Watson, who had her case in charge, that the vomiting ceased in three or four days and she began taking nourishment, but that she remained rather stupid and had occasional rises of temperature and in twenty-one days after the uterus was emptied died after a paroxysm in which her fever ran up to 105.5°. The urine in this case showed a trace of albumen which was accounted for by the presence of pus.

## DISCUSSION ON PAPERS OF DRS. JORDAN AND TALLEY.

Dr. Charles H. Mayo, of Rochester, Minn.—In considering the toxemia of pregnancy, we are thinking mostly of the end-results. It would be interesting if we could get at the real primary cause of why this albuminuria, for instance, taking the type of trouble referred to, should occur.

With regard to the action of the ductless glands, we are learning a little about it, but there is much more that we do not know. The thyroid, for instance, in the baby develops through the influence of the thyroid of the mother dilating capillaries and promoting growth. If the child is born without a thyroid of its own, it fails to grow, and continues as a dwarf, as a cretin. Later in life, with the loss of the thyroid, we get myxedema in a large proportion of cases; then the over-activity of the gland in Graves' disease comes, and with the effect of changes from the unopposed suprarenal gland, so that an interesting fact was recently reported in one of the foreign clinics, namely, the examination of 130 pregnant women at about the eighth month, one of the signs of pregnancy being enlargement of the thyroid. Of 108 women who had enlargement of the thyroid, 2 of them had albuminuria, and they had Bright's disease before they became pregnant. Of the remaining 22 patients, 16 had albuminuria, and 6 had eclampsia, but did not have a perceptible enlargement of the thyroid at this particular time, and it brings up an interesting question for the obstetrician to discuss, and that is with regard to hypertrophy of the thyroid of the mother, which influences the child and metabolism of her own body.

Dr. J. Whitridge Williams, of Baltimore.—I have listened with great interest to these two papers, and there are one or two points to which I desire to call attention. In the first place, the observations that have been made in the last few years have shown clearly that the vomiting of pregnancy is not as harmful as it was formerly supposed to be. In the second place, these observations have shown that when these women die they do not die from starvation, as was formerly believed, but die from a deep-seated toxemia, with lesions of the liver analogous to those found in other parts of the body. The great majority of women who suffer from the vomiting

of pregnancy do not have the toxemic variety, and it is a matter of great importance to distinguish whether or not the patient has this form or some other form which is attended with danger. We have worked along that line, and, roughly speaking, we distinguish two main groups, namely: neurotic and toxemic vomiting. These patients rarely die from vomiting; indeed, they ought never to die if properly treated; while the toxemic cases die in large numbers, no matter what method of treatment is pursued. We have at hand a means of diagnosis of these two conditions. We cannot tell by looking at them. We see two women who are equally sick; one of whom will recover, while the other will die; one having the neurotic, the other the toxemic variety. We have to distinguish the two groups by examining the urine and the ammonia nitrogen. In the vomiting cases the ammonia nitrogen is normal; while in the toxemic cases it is largely increased. Having made the diagnosis as to which group you have to contend with, your treatment is settled. You can cure the neurotic cases in from twenty-four to seventy-two hours by suggestion.

As to the other point which Dr. Jordan referred to, I am inclined to believe that eclampsia is a totally different toxemia from the vomiting and yellow atrophy type. I can only endorse what he says, that there is absolutely no point or points in common between the two diseases, with the exception that both are likely to kill the patient.

My grounds are two-fold: In the first place, we have a totally different condition in the liver. As Dr. Jordan has told you, in the eclamptic toxemia we have a peripheral necrosis; in the other, vomiting and acute yellow atrophy toxemia, we have central necrosis, and anyone who has seen these conditions under the microscope knows that they are just as marked as any two lesions one can find in the same organ.

I simply add my voice to what Dr. Jordan has said, namely, that it is a great error to speak of the toxemia of pregnancy. There are many toxemias. We have the pre-eclamptic and the eclamptic toxemia; we have the vomiting toxemia, and this gives rise to acute yellow atrophy. There are other toxemias of which we know nothing. What we want to do in these cases is to learn about the individual conditions. Those who look upon all toxemias as alike base their conclu-

sions entirely on theory. Because a woman dies in the latter part of pregnancy in coma and possibly in convulsions, therefore, she has eclampsia. If they were to make an autopsy in one case, they would find a central necrosis; while in the other they would find peripheral necrosis. In the same way they assume that there is no demarkation between the two. That is incorrect. The difference is sharply marked, and whenever we see a woman dying and getting peripheral necrosis in the liver, regardless of the clinical history, we are justified in claiming that she has eclampsia. If she has a central necrosis, she does not die from eclampsia. There is no sense in grouping all these cases together in a single category in speaking of the toxemias of pregnancy. If we do, then we are confusing the matter which is already more than sufficiently complicated. To my mind, to attempt to group them all together into a single group is quite as illogical as to group all septicemias together, and you know what a difference it makes whether we have a septicemia due to the streptococcus, the gonococcus, the typhoid bacillus, or what-not. We know very little about this matter. We know very little about the general metabolism of pregnancy; but we do know that there are marked changes in normal women, and diminished resistance and unstable equilibrium under the influence of pregnancy may lead to deep-seated changes under the circumstances.

I wanted to bring out those two points. We have a type of vomiting of pregnancy which is accompanied by marked degenerative changes in the liver and closely allied to acute yellow atrophy. In the second place, we do not have merely one toxemia of pregnancy; we have a number of them which should be clearly differentiated, and we want to know two things—what causes it, and how to cure it.

Dr. Jordan (closing).—I was very much interested in the remarks of Dr. Mayo about the thyroid. If I remember correctly, he stated at the Atlanta meeting of this Association that there was an apparent balance between the effect of the thyroid secretion and that of the suprarenal bodies, although that statement did not appear in the Transactions. I would like to know whether or not I understood him correctly.

Dr. Mayo.—I believe I made that statement. I said there was an apparent balance among other important functions performed by both glands.

Dr. Jordan (resuming).—It is interesting, then, to speculate, if speculation is allowable, on the etiology of the lesions of eclampsia. In the first place, as stated by Dr. Mayo, and also mentioned by another writer (Edgar), in a recent article, women whose thyroids do not enlarge during pregnancy are prone to have eclampsia. Inasmuch as the lesions have been stated by Dr. Williams and Dr. Schmorl to be secondary to thrombotic processes in the interlobular spaces, Would it not be possible for the thrombosis to be brought about by the over-action of the suprarenal secretion acting as a ligature around the small vessels. We see somewhat the same changes in ergotism, for instance, which is supposed to have a similar effect on the small vessels, causing gangrene.

Dr. Williams' remarks on the ammonia test in the diagnosis of the toxemic variety are certainly interesting, but it is to be regretted that it is so difficult, and that such a complex chemical procedure is not available for the general practitioner. To my mind there is no doubt whatever that an individual with insufficient liver is almost certain to have increased ammonia in the urine? While this has been brought out in the literature, there is no lack of experimental evidence to support it. If we divert part of the portal blood from the liver by an Eck fistula, we get identically the same findings in the urine.

# CONSERVATIVE METHOD OF MANAGING UNDESCENDED TESTICLE.

By Joseph Price, M.D.,
*Philadelphia.*

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Methods for the permanent fixation of the undescended testicle in its normal or physiological position, the scrotum, are not numerous. Unfortunately most surgeons have been largely influenced by the literature and by common surgical practice. The early and common practice of removal, because atrophied and considered physiologically useless, has guided modern surgeons to continue the practice and not devise some ingenious plastic method of anchorage or fixation in the scrotum. The method of C. B. Keetley, described in the London *Lancet*, July 2, 1905, in his series of 25 cases on that of fixation of the gubernaculum to the fascia lata of the thigh is the only operation recorded in which the authorities claim good and pleasing results. The operation is generally satisfactory to the patient. Again, strangulation of the testicle cases which are reported, cysts with retained testes, cyst or hydrocele, are to be found carefully recorded throughout the literature of the subject. In *Guy's Hospital Reports*, vol. liii, 1896, p. 282 (574), cysts with retained testicle, 284 (999) strangulation of testes. I have twice removed the right testicle, strangulated by rotation I found in the inguinal canal, and I have twice anchored the testis by what I call the "four-in-hand" method. The Bassini operation, when practicable, the tunic opened, the cord freed, finger or forceps passed to the most dependent

point of the scrotum—scrotum buttonholed at that point—a generous loop of gauze seized by the forceps and drawn back to the testes. Cast above and about it and anchored beneath by a gut suture and then pulled well down into the scrotum and the canal closed by any of the hernia operations is most suitable or preferred. I thought of this method because I had practised, in part, a similar procedure in imperforate anus.

Inguinal colotomy, finger passed to rectal limit, dissection and fixation of the rectum—thus completing a simple procedure and avoiding a blind dissection.

Following is a brief *resume* of the more or less conspicuous literature: Among the first were reports by Vicq d'Azyr commenting upon Jean Hunter's report, published in 1762, and read before the Hist. Acad. Roy. Soc., Paris, 1784, upon findings and the position of the testicles in the fetal abdomen and its descent and obliteration of the vaginal tunic. Biddell, in London *Lancet*, 1893–94, says the treatment of undescended or partially descended testicle does not appear to be very definitely laid down in any work on surgery.

He describes an operative procedure as follows:

The testicle, after having been brought down to the bottom of the scrotum, is held in place by a knotted-silk thread and is attached by means of an elastic drainage tube and a safety tube to a wire cage. Van Severingen, in *Univ. Med. Mag.*, Phila., 1892–93, describes the case of a boy, aged thirteen years, feeble-minded; strangulation was due to twist and incision in inguinal canal. Keetley, in *Trans. Med. Society*, London, 1893–94, advises two skin wounds made in the scrotum and thigh (adjacent), and the wounds sutured to fibrous tissue found attached to the testes and epididymis; sutures being made to traverse the tunica vaginalis. Lockwood, in *Trans. Patholog. Soc.*, London, 1896–97, reports a case of a man, aged forty-two years, father of family, with congenital hernia on left side all his life. The hernia descended to the scrotum in the left testicle in the inguinal

canal but it was easily slipped into the abdomen. The testicle was removed, and sections made and no spermatozoa found. Smith, "Functional Efficacy of Retained Testicle," in *Guy's Hospital Reports*, 1896, vol. liii, is an exhaustive and one of the best of the early works on physiology, etc., of the undescended organ. McDonagh, of Sydney, in *Australian Med. Mag.*, 1901, vol. xx, did a right castration, with marriage and children following. Alexander, in *Annals of Surgery*, Phila., 1900, xxiv, describes an operation of freeing the vas deferens from the cord and pushing back the peritoneal reflection as far as may be necessary to restore the latter to its normal length; the other point consists of adhesions (at the internal ring principally) of the fascia arising from the transversalis and cremasteric muscles; completion same as the Bassini. Bucknell, in *Clin. Jour.*, London, 1901, xix, says hernia is found in 10 per cent. of all cases. He advises manipulation in infants regularly; operates after two years; if this fails, no attempt made to save testicle of the body, and epididymis is widely separated; or if it is necessary to cut all vessels of the cord in order to bring the gland into the scrotum, and no operation except castration if patient is past puberty. Katzrustein, in 1902, in *Deutsche medicinische Wochenschrift*, reports several cases with attachment of the scrotum to the thigh. Ploc, in 1902, in *Zeitschrift f. Heilkunde*, vol. iii, reports 2 cases, thirteen years of age. In the first case the testicle could be brought below the symphysis but a short distance; the cord and connective tissue were stitched to the periosteum; the testicle remains today in the upper third of the scrotum. In the second case both sides were undescended; the same operation was followed by perceptible growths of the testicle, and general benefit was noted.

Duchesue, from Ploc's article, operates from the twelfth to the twentieth years, and thinks it unnecessary before the twelfth year. Tinotti agrees with Besancon and operates after the eleventh and twelfth years, since experience teaches

that the testicle may descend spontaneously before that time. Péon was able to prove the presence of spermatozoa after a two-year operation.

Nicholson, in *Annals of Surgery and Gynecology*, St. Louis, 1899, reports a case where he invaginated the scrotum and stitched it to the testicle and epididymis, and then used a compress. The testicle increased four times in size.

Monsarrat, in *Liverpool Med. and Chir. Jour.*, 1902, gives a good discourse and advises letting it alone before the second year when without symptoms. Numerous papers appeared about this time by Odione, *Annals of Surgery*, Phila, 1904–1905; Tulby, of London, 1904–1905.

Harrington, *Amer. Jour. of Surg. and Gyn.*, 1904–1905, reports a case in a man, aged twenty-three years, of strangulated hernia and undescended testicle and appendix, continuous with hernia sac as far as Poupart's ligament. Castration was done on account of the short length of the cord. Keetley's article, published in the London *Lancet*, 1905, described an operation giving best results, and mentions using gauze as a tractor. Ware, in an article in the *Amer. Jour. Dermat and Genito-urin.*, St. Louis, 1906, suggests a different operation; also McLean, in *American Medicine*, Phila., 1906. Castration has been performed for malignancy of the testicle, and many cases reported. Wallace, in London *Lancet*, 1898, also *St. Thomas' Hospital Reports*, 1899; Wyeth, *New York Med. Jour.*, 1905; Darby, in London *Lancet*, 1904, i, etc. Tuberculosis, hydrocele, and other complications have also been encountered.

## DISCUSSION.

Dr. Robert T. Morris, of New York City.—We are all glad to hear Dr Price speak on this subject. The Bevan operation has given us, in my experience, the simplest resource for restoring the undescended testicle to its normal position. The idea, in brief, is this: The vas deferens and

its accompanying artery are tortuous; the connective tissues and the numerous smaller vessels are resisting. The connective tissues and the smaller vessels and peritoneal attachment are, therefore, severed, leaving the elastic and tortuous vas deferens and its accompanying artery free. When these are free, the testicle can be brought down two, three, or four inches, and will remain in the scrotum where it is dropped; you have to make an opening in the scrotal planes with the finger, and the testicle will lie there from its own weight, dropped two inches below its site, if the operation has been done according to the Bevan method.

Dr. Samuel J. Mixter, of Boston.—It seems to me the whole question of operation for undescended testicle comes to this: If a man has one undescended testicle and you operate on him for a hernia or discomfort from the testicle, take it out. A single testicle is just as good as two. There is no question about that. Above all things do not return an undescended testicle into the abdominal cavity, as has been done in some cases, because we all know and I think it has been very well proven, that a great many undescended testes become sarcomatous, and that is one reason for not saving them. If a man has one testicle and it has descended, leave it and take the other out if it is necessary to do anything. But my experience has been that testes which are stripped of the vessels, and so on, and brought down, while they stay there they become atrophied, and they are just as well out as in except for cosmetic purposes.

Dr. Maurice H. Richardson, of Boston.—With reference to the tendency to strangulation of the testicle which was spoken of by Dr. Price. His accident is, I think, one of the dangers of undescended testicle; but, the chief danger is malignant degeneration—sarcoma. I have seen three strangulations of the testicle: one of which took place in a testicle at the external ring; in the other two I am inclined to think that there was an abnormal condition of attachment—a pedicle more or less limited in breadth and long enough to permit easy rotation. It is so difficult for a normal testicle to become strangulated that strangulation is *per se* evidence sufficient to raise the suspicion of abnormality. I have seen some three or four cases of sarcoma of the testicle undescended from the abdominal cavity. In a very recent case both testicles were similarly diseased; the right in the scrotum and the left

high up in the abdomen. So many cases of sarcoma of the undescended testicle have been observed at the Massachusetts General Hospital that it is a serious question whether all cases of undescended testicle with obscure abdominal symptoms should not be explored in the very beginning.

Dr. J. M. T. Finney, of Baltimore.—I have had considerable experience recently with these cases, and I am sorry to say that it has not been so satisfactory as that of some other gentlemen who have spoken. I have tried all the methods with which I am familiar. At the present time my practice is, that if there is a hernia present, I operate, and in a large percentage of cases remove the testicle if the trouble is unilateral. If there is no hernia present I do not operate. Where we have a totally undescended testicle in a child, and both sides are affected, the question arises, What shall we do? My results (although I have followed as carefully and conscientiously as I could the published reports of Bevan's method and those of others) have been unsatisfactory. It has been comparatively easy to get the testicles down, but in the vast majority of cases so much traumatism has been inflicted that epididymitis and atrophy of the testicles followed. When a testicle has become atrophied it is functionless, and becomes a menace on account of the possible development of sarcoma. But, it does seem as if it were better to preserve the testicle in children because of the internal secretion and its effect upon the development of the child. So, my custom has been to advise no operation where there is a totally undescended testicle, but to wait until the child has pretty well gained its growth and development, and then if a hernia develops and there is, as Bevan has pointed out, a potential hernia in these cases, then one can operate or not as seems best. Perhaps my experience has been different from that of other members.

Dr. Robert S. Hill, of Montgomery, Ala.—I am very much surprised at the discouraging reports made here today. I have seen several cases of undescended testicle operated on by my associate, Dr. L. L. Hill, and with the exception of the first he experienced no great difficulty in bringing the organ down. The result in each case was entirely satisfactory. Without an exception the testicle commenced to develop in a short time after it was put in the proper place and soon attained a normal size. One case was particularly interesting

from the standpoint of nervous manifestations. This patient was six years old and had epileptic attacks. It was noticed that these attacks were associated with a sensitiveness in the undescended organ. The testicle was placed in the scrotum and the next day his temperature rose to 106° F. where it remained for some three or four days after which it gradually declined to normal. The epileptic seizures disappeared and the child regained perfect health. So, considering the experience of my associate, Dr. L. L. Hill, I have been lead to believe most of these cases can be relieved.

Dr. Price (closing).—The experience of Dr. Finney corresponds beautifully with that of prominent authors on the subject as to development and retrograde changes. We cannot take much liberty with a diseased organ. There is a degree of plasticity and of retrograde change taking place as well as softening which most of the authors allude to, and it is important, of course, to free all adhesions and bring the testicle from above down into the scrotum. In a small percentage of cases, surgeons have found an associated hernia nearly always on the left side. Nearly all authors dwell on the softening of the organ, and they likewise allude to the possibility of accidents by traumatism and undue force and inability to get the testicle down. A London surgeon, who makes a dissection, opens the scrotum, brings the testicle through and opens the thigh, anchoring the testicle to the fascia lata. In one case it is my impression that the collateral circulation had something to do with the development of that testicle; but, as to the anchorage in that case, it would seem that if we were to get anything in the shape of improvement in its development and function we should attach the testicle to something more vascular than fascia or muscle.

# GALLSTONES WITHOUT SYMPTOMS AND SYMPTOMS WITHOUT GALLSTONES.

By Maurice H. Richardson, M.D.,
*Boston.*

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Measured by the percentage of correct deductions, the evidence upon which *most* diagnoses of gallstones are made is reliable—more reliable perhaps than the evidence upon which the diagnosis of any other deeply seated and impalpable lesion is based.

Measured by the percentage of correct deductions, the evidence upon which *some* diagnoses are based is almost indisputable. Brief attacks of pain and jaundice, with a tender and perceptible gall-bladder, make a group of symptoms so unmistakable that the deduction of gallstones is inevitable, in fact, any other deduction would be unreasonable. Brief attacks of pain, with a tender and perceptible gall-bladder, even without jaundice, will point almost indisputably to a gall-bladder origin and to gallstones. In the great majority of cases the diagnosis of gallstones is easy. Indeed, the diagnosis in the typical case needs no further comment.

In the atypical and obscure case the diagnosis is much more difficult. Fortunately, the increasing frequency of explorations in the right upper quadrant has demonstrated so often the connection between these obscure and atypical symptoms and their pathological cause that we can already, in the great majority of cases, make a correct inference, if not of gallstones, at least of a mechanical condition which for its relief demands mechanical measures.

The history and physical examination before exploration, with the anatomical demonstration of the biliary tract and adjacent viscera, afford both the physician and the surgeon indisputable control over their diagnosis. Such an opportunity as surgery now affords of studying the pathology of the whole abdomen in general, and of the right upper quadrant and epigastrium in particular, is not as yet appreciated by either physician or surgeon. The importance to the physician of a personal observation of the truth or error of his diagnosis, as demonstrated at operation, appeals to but few medical diagnosticians, and yet the opportunity of renewed physical examination, under complete anesthesia, is not one to be lightly thrown away. The importance to the surgeon of a thorough study of anatomical appearances during life is vital, for upon a correct interpretation of the anatomical changes depends the choice of operation and its success.

With such opportunities as are now afforded, a careful study is indicated of all symptoms which even remotely suggest gallstones, and especially atypical and obscure symptoms of the right upper quadrant, for which, until operations became frequent, no adequate cause could be predicted. Those symptoms, therefore, which suggest strongly but not markedly gallstones, but which may prove to be of quite different origin, must be seriously considered lest, under the narrow limitations of a too positive diagnosis, an unnecessary operation be performed, or an imperative one be withheld.

Excepting lesions of the female pelvis and perhaps of the appendix, the most important diseases of the abdomen are situated in or near the right upper quadrant of the abdomen where they manifest themselves by symptoms persistent, disabling, and unresponsive to treatment. Organic diseases of the stomach, duodenum, liver, gall-bladder, pancreas, right kidney, and hepatic flexure of the colon present symptoms of the widest diversity, some of which are commonly

found in all; some of which are occasionally found in all; and some of which are found in but one. Pain, for example, in some of its forms is common to all: loss of weight, loss of appetite, and other constitutional symptoms may be present in all. But jaundice is present only in diseases which obstruct the biliary flow; hematemesis, only in diseases which involve the esophagus, stomach, and duodenum; hematuria, only in lesions of the genito-urinary tract.

In the great majority of these lesions, especially at the operable stages, it can be taken for granted that the chief symptom will be pain in some form or other. The more pronounced any sign pointing to the cause of pain, the more positive the diagnosis, but the more advanced the disease. Gastric spasm, for example, locates the cause of pain in the pylorus, just as jaundice locates it in the biliary tract, or blood in the urinary. In the diagnosis of gallstones in their earliest stages without jaundice; in the diagnosis of pyloric growths without gastric spasm or hematemesis; in the diagnosis of renal tumors without hematuria—the chief symptom is usually pain, and the character of that pain is the only guide at that early stage when, in neoplasms, there is a chance of cure, or, in gallstones, an uncomplicated biliary tract. At a stage thus early, errors in diagnosis must occur; errors in indications for operation must occur; errors of judgment between the imperative and the needless operation must occur.

Fortunately, however, even if at operation no gallstones are found, other lesions are usually demonstrated and successfully remedied. It is the rarest of exceptions when the operation demonstrates only its own uselessness. It is excessively rare to find neither lesion nor relief, or to make the patient worse than he was before operation.

These remarks are illustrated so frequently that no prolonged search for illustrative cases is necessary. The number of patients upon whom a needless operation is performed is very small, and yet improvement in our results demands

that these cases be especially emphasized and avoided. But if we emphasize errors of commission, on the other hand we must emphasize with equal force the errors of omission. It is not difficult to find examples of both.

A gentleman of sixty-two years, after a very severe attack of left abdominal pain—with vomiting, rigidity, and distension, *without tenderness*—symptoms which disappeared completely, passed bloody urine. No more natural inference could be drawn than that of left renal stone and reflex abdominal symptoms. A second attack proved fatal. Autopsy showed perforation of the posterior wall of the stomach with stone in the left kidney. The diagnosis was right as far as it went, but it did not go far enough. The man had the benefit of the opinion of clinicians in whom I have the greatest confidence. At the second attack he was in such desperate straits that it was clear to all that his only chance lay in palliation. Could the real lesion have been detected before the second attack, he might have been saved.

A woman of forty-one years (vol. xxiv, p. 63) had suffered for two years from abdominal pain. In 1897 she was operated on: the appendix was removed, and also the right tube, which was dilated to about the size of a mandarin orange. In 1901 she was operated on for hernia in the scar. Two years later she died after an operation for gallstones, the details of which I do not know. Were the gallstones the major lesion, and the appendix and tube the minor?

A woman of forty-two years (vol. lxii, p. 57) was operated upon for pain supposed to be due to gallstones. I removed several gallstones, but the pain persisted and was finally accompanied by visible and violent gastric spasm. Cancerous stricture of the jejunum was found. This had not been detected at the first operation. Thorough extirpation has so far given complete relief.

A man of forty years (vol. lx, p. 125) was operated upon for supposed appendicitis, diagnosticated chiefly from right-sided pain. Operation brought no relief. There is

now, for the first time, bloody urine, and the diagnosis of renal stone is probably correct. For pain in the region of the appendix I have operated many times, even when the diagnosis was not positive. In some (very few) of these cases other diagnoses have later become apparent, and the results, as a whole, have been of the most gratifying kind. But, as I have said, these few exceptions in the present splendid development of surgery are the only things left for us to study and improve upon.

Under the diagnosis of gallstones, even of the most positive character, lie possibilities of error; and no one knows this fact so well as the operating surgeon. Indeed, no one sees, as he does, the intimate relation between cause and effect, and no one can possibly be in a better position to avoid errors in diagnosis and in the indications for treatment. But, be he ever so experienced, so accurate in his observations, so logical in his deductions—cases will arise in which he will find himself completely mistaken. Instead of gastric perforation, he will find acute cholecystitis (vol. lxii, p. 32); instead of gallstones, he will find chronic pancreatitis (vol. l, p. 90); instead of pancreatic disease, he will find gallstones; instead of gall-bladder abscess, he will find renal abscess; instead of tumor of the gall-bladder, he will find a hypernephroma (vol. xliv, p. 155); instead of gallstones, disease of the pylorus; instead of gallstones and cholecystitis, perforation of duodenal ulcer and general peritonitis. Furthermore, he will find not only the disease that has been diagnosticated, but other lesions as well, and lesions perhaps more likely to be the cause of the symptoms than that for which the operation was undertaken, as in the case cited of appendix and tube with gallstones; jejunal cancer and gallstones (vol. lxii, p. 57); pancreatic cancer and gallstones (vol. l, p. 90); pyloric adhesions and gallstones (vol. xl, p. 171); intestinal cancer and gallstones (vol. lix, p. 114); hepatic cancer with gallstones (vol. lxii, p. 67, and vol. lx, p. 173).

The most common experience, however, is that in which, after a very positive diagnosis of gallstones, nothing is found. Were not an adequate explanation at hand—the fact that the last gallstones may have been expelled at the last attack of pain—this experience would be indeed hard to explain. No doubt this explanation is a satisfactory one in some cases. Indeed, it cannot be said in any case that this may not be the right one. But, with multiplying observations in cases in which there has been a long history of repeated attacks of pain, without the discovery of gallstones in the stools, without jaundice or other evidence of a protracted passage through the common duct—with multiplying observations of such cases it seems to me impossible that this explanation can be accepted as the right one. We must, therefore, consider the significance of symptoms which suggest gallstones by some kind of pain in cases in which no gallstones are found; we must also consider the significance of the absence of symptoms when gallstones *are* found, perhaps filling a gall-bladder which, by its thickening and contraction, expresses the slow and painless changes of many years.

Why is it that in some cases a single stone causes pain of the most excruciating kind, whereas, in others many stones cause none? An answer immediately suggests itself—the difference in the impaction; its situation; the size of the stone. But have we not learned that this is much too easy and superficial a view of the subject? In one instance, at least, the attacks of pain recurred after the removal of about a hundred stones (vol. lxiii, p. 43). The attacks became frequent and unbearable. Inasmuch as I had many times observed pain after cholecystostomy and drainage, I looked upon this case as similar to those in which there had been pain which later disappeared. Dr. Jones operated on this patient for me, and removed the gall-bladder. He found it adherent to the old cicatrix and surrounded by adherent omentum. It was dilated and

contained about six ounces of dark, almost black, thickened bile. The adhesions extended down about the cystic duct, but no definite band or kink was found. The head of the pancreas was thickened to about an inch and a quarter. No stone could be found after prolonged search. The cystic duct, when opened, proved to be very tortuous. The gall-bladder wall was somewhat thinned by the dilatation, especially at the fundus where it had been drained. The patient made a good recovery. In this case, however, there was adequate cause for cholecystitis either in the presence of gallstones before the first operation, or in a possible lesion of the mucous membrane during tube drainage.[1]

I have for many years, but particularly of late, been impressed by the value and interest of diagnosis as a healthy and stimulating intellectual exercise which highly cultivates the powers of observation and deduction. The surgeon has opportunities of testing and verifying not only the significance of his histories, but the results of his own physical examinations and those of others, and especially the deductions that he draws from the evidence at hand.

Of all the areas in the body (excepting perhaps the field of cerebral diagnosis, where correct deductions from accurate observations offer the most extraordinary opportunity for the highest intellectual endeavor) none equals, in the variety and frequency of disease, the right upper quadrant of the abdomen, for there lie possibilities of widely differing lesions with symptoms more or less common to all. The similarity of symptoms, especially in the early and best operable stages of all these lesions, makes correct diagnosis extremely difficult; but it makes correctness of diagnosis

[1] Not long after this operation the pain recurred with renewed violence. While we were discussing the question of what was bets to do, the patient passed by rectum a stone which we hope is the last one. This case illustrates what the probable cause of postoperative gallstone pain is—an overlooked stone or fragment.

and consequent efficiency of treatment of inestimable value to the patient. I cannot but feel, therefore, that at the present time there is no task set before the surgeon that is more productive of good than that of the early recognition of those lesions of the right upper quadrant, which in their early manifestations are cured with little if any danger, but only with great and unavoidable danger in their later. But in emphasizing the importance of diagnosis in the right upper quadrant, as well as elsewhere, one must not in his enthusiasm forget that, delightful and satisfactory as it may be to make correct deductions from accurate observations, internal diagnosis is an exceedingly difficult and elusive problem, and the diagnostician must be particularly careful in making those diagnoses which condemn the patient, without even an attempt at relief, to an inevitable death. Even though the observer, has through years of general practice, of the practice of general surgery and of abdominal surgery, acquired great nicety of diagnostic accuracy, yet—skilful and logical though he may be—he must be on his guard lest he make such an error on the side of conservatism and inoperability as to remove the patient's only chance of cure.

We are told that, at the end of a long life devoted to the Greek definite article, the professor's chief regret was that he had not confined himself to the dative case. So may we sometimes feel, in spreading our observations over the wide field of surgery—we may regret that we have not devoted our lives to the abdomen, or to a single lesion in the abdomen. Seriously, however, the best way to study a particular subject is to approach it through subjects allied to it—through general subjects. The best education for the surgeon is general practice; for the abdominal surgeon, general surgery.

In the subject under consideration, I am convinced that further progress can best be made by frankly admitting the errors of diagnosis and treatment, but more especially the errors of diagnosis. In gall-bladder surgery, therefore, I

wish to call attention to various errors in diagnosis and their results, whether beneficial or injurious, whether influencing the course of the real disease in a manner favorable or unfavorable.

These errors have come under my personal observation in cases of my own, and are errors for which I have been partly if not wholly responsible. It seems but fair to myself and others concerned in them to explain that they are very infrequent. They are selected from many hundred cases, operated and unoperated, in which there has been a correct diagnosis in an overwhelming majority. Indeed, were it not that the correctness of predictions has been almost invariable, I should not have been so deeply impressed by the occurrence of mistakes or so eager to prevent them.

The great deterrent to conservatism, especially in abdominal surgery, is the difficulty of demonstrating beyond a doubt that the greater, rather than the lesser, evil is present; that, for example, when gallstones are diagnosticated there is a lesion more serious than that of gallstones, rather than one less serious or even no lesion at all. Estimated by the chagrin which the surgeon feels, perhaps the worst error is that of performing an absolutely unnecessary operation, by which, however, the patient receives no injury; estimated by his remorse he feels that there is none greater than that following a perfectly unnecessary operation by which the patient's life is sacrificed, unless, perhaps, the self-reproach is as great when he has withheld an operation essential to life; estimated by the evil to the patient, none can be greater than the infliction of a fatal operation for a cause which does not exist, for the withholding of an operation does not always mean death—few lesions are essentially fatal without relief, and those few seldom unrecognized. Estimated by the evil to the patient when death follows, the infliction of an unnecessary operation or the withholding of a necessary one have the same result; but, estimated by the probable evil there is of course no comparison because the great

majority of unnecessary operations at least do no harm, whereas, the great majority of lesions demanding operations are, without that operation, fatal.

If the surgeon would avoid chagrin, self-reproach, and remorse, he should take that course which in the aggregate of cases proves of the greatest benefit to the patient—the operative one, for the safety of surgical operations is so great today that little injury is likely to follow an unnecessary operation in which the doubt of diagnosis is raised; while so fatal are the lesions in which the doubt of diagnosis is raised—more especially those in the vicinity of the pylorus—that the withholding or postponing of operation is almost necessarily evil.

Estimated by the good to the patient, the application of surgical measures to the lesions which may, however doubtfully, demand them, is vastly greater than the withholding of such measures on the ground that no lesions exist to demand them. Estimated by the sum total of evil and chagrin, self-condemnation, and remorse, the worst are comprised in inflictions of unnecessary operations and the withholding of necessary ones. Estimated by the least evil to the least number, the infliction of the unnecessary operation is incalculably less. Estimated by the greatest possible good to the greatest number, successful effort lies in the elimination of the unnecessary operation and the selection of the necessary one.

Successful effort then demands that one cultivate to its highest perfection the powers of diagnosis.

I must ask the indulgence of this Association for emphasizing facts which are indeed truisms; but even truisms may be neglected or forgotten. The tendency is toward hasty, imperfect, and inaccurate diagnosis, because operative diagnosis is so easy and unmistakable. This tendency is surely a pernicious one, and one to be resisted with all our might. No greater reproach can be brought against the surgical art than that it is only an art, and that, as ordinarily

practised, it can never rise in the hands of the surgeons themselves to the plane of a science. Whenever time permits, the proposed operation should be based upon the most careful and exhaustive study of the patient; or, still better, no operation should be performed until that operation, by a positive diagnosis, has been found necessary. Every lesion should be regarded as one not demanding or justifying operation until, in the particular patient, that demand has been completely justified. The exploratory laparotomy, for example, should be reserved for those cases in which security of the patient demands *certainty* of diagnosis, and those in which there is everything to gain, and little to lose, by exploration.

The diagnosis of gallstones is usually easy and beyond doubt. The cases which require study are those comprised in my title, for those are the ones in which errors of commission and of omission are to be found. One will occasionally find nothing abnormal when he confidently expects to find gallstones. In some cases, though he will find no gallstones, he will find other lesions, like chronic pancreatitis or pancreatic cancer; in others, the bile will be thick, black, and tenacious, without gallstones—cases in which cholecystostomy and drainage are followed by complete and permanent recovery (vol. xlviii, p. 117); in still other cases (vol. lvii, p. 167, and vol. lix, p. 102) there will be an unchanged gall-bladder and unchanged bile. When, in the doubtful case, the diagnosis of gallstones seems, on the whole, the most probable, gallstones will be found in the majority of instances.

The errors in diagnosis are met with not only when no physical signs are present, but when the history is not characteristic, or at least not in accordance with our present and recent ideas; when, for example, the only complaint is that of an obscure, non-localized pain, a discomfort, a distress, a grinding, boring sensation (of which the patient will say that it is hard for her to tell when it begins, when it ends, or

just where it is located). In a few instances the pain may be characteristic of gallstones, as to onset, situation, direction, intensity, persistence, and termination. After a single attack of characteristic gallstone pain, failure to discover any stone or abnormality in the gall-bladder is quite conclusive that the only stone or stones were actually expelled. Indeed, the more characteristic the attack is of successful ejection, the less will be the anatomical changes in the biliary tract. On the other hand, the more obscure and uncharacteristic the attack, the greater the anatomical changes.

In many of these cases of extensive anatomical changes the history will often be devoid of symptoms suggesting in the least the presence of a gallstone origin. When I have found gallstones in the course of other operations, and have been over the history in the light of the demonstrated facts, I have seldom found a characteristic history of gallstones. True, I have found complaints for which the demonstrated presence of gallstones and their anatomical effects offer an explanation, and doubtless the correct one; but except in certain cases this explanation, in the light of previous knowledge and experience, would have been regarded as absurd. Indeed, in the light of present knowledge, such histories would still be regarded, by some at least, as totally inadequate for a reasonable diagnosis of gallstones or for a justifiable operation.

In some cases I have been obliged to make the most searching cross-examination to detect a single suggestive symptom. I do not refer to gallstones harmlessly suspended in a gall-bladder full of bile; but to gallstones filling to its utmost capacity a gall-bladder not materially changed, and to gallstones so tightly grasped by a thickened and contracted gall-bladder as to be deeply embedded in its mucous membrane.

In comparing the evils of an inaccurate diagnosis, we are forced, I think, to the conclusion that our failure to recognize early the presence of gallstones is by far the greatest

evil that attends our treatment of diseases of the biliary tract. An operation which demonstrates nothing organic in the right upper quadrant is a chagrin, not to say a humiliation; but it has the virtue of at least demonstrating the absence of organic disease—a by no means slight merit—and it has the virtue of comparative safety. I am not so sure that the satisfaction of knowing, beyond a doubt, that there is no anatomical cause for persistent and disabling symptoms in the right upper quadrant is not well worth the risk of exploration. Considering the numerous causes of symptoms in that anatomical area from diseases other than gallstones, the satisfaction of an unsuccessful search for organic lesions is all the greater.

It is a difficult task to satisfy the patient, family, and friends with the account of an unnecessary surgical operation, and especially a fatal one. We probably all have felt that, by the perversity of fate, our worst disasters have followed unnecessary, or at least not urgent operations. When a patient dies from the anesthetic in a tooth extraction, in an operation for hemorrhoids, or for a small shoulder lipoma; in a simple ovariotomy, appendectomy, or an unfruitful exploration; when she develops pneumonia, pleurisy, or a fulminating-wound infection; when she bleeds to death from an operation upon a torn cervix; when death takes place from suppression of urine, acute parotitis, a spreading phagedena, a gas infection, a pulmonary embolism, tetanus, or even typhoid fever and other infectious diseases—when these things do occur, we have all felt the inadequacy of explanation or excuse.

So in operations for supposed gallstones in which nothing is found, we are forced to admit to ourselves the commission of a surgical mistake, one which would never have been committed were our powers of diagnosis more reliable. But can we ever hope to be free from occasional errors?

The only errors from which we can and ought to be free are those which depend upon inadequate study, upon haste

and carelessness in observation, and upon precipitancy of deduction. But we must not forget that these causes of error may lead the conservative man to overlook serious diseases quite as frequently as they may tempt the radical to explore for diseases which do not exist.

Under the heading of gallstones without symptoms may be classed those lesions in which there is in reality a gallstone origin, but in which the symptoms are dependent upon remote effects. Acute intestinal obstruction, for example, is occasionally caused by gallstones, and this fact must be borne in mind in the diagnosis of lesions causing acute obstruction. In the 2 cases which I now recall, a gallstone cause was not considered at all. The diagnosis of complete obstruction was so evident that immediate exploration was made and showed the gallstone cause. In the first case, however, no previous symptoms suggesting gallstones had ever been noticed; even the most searching inquiries were negative, and not a moment's pain or discomfort was described. I cannot but think, however, that, with our present experience we should have elicited at least a history of indigestion. In the second case, that of a woman of sixty-eight years (vol. lxii, p. 45), my diagnosis was chronic intestinal obstruction, probably from cancer. Dr. Clara J. Alexander found a gallstone an inch in diameter, an inch and a quarter long, and three inches in circumference, completely obstructing the small intestines. In going over this history before making my diagnosis I paid little or no attention to two facts. This history (taken from the records of the New England Hospital for Women and Children) was as follows: "Patient aged sixty-eight years. Family history negative. No acute illnesses, except children's diseases; grippe seven years ago; headache rather frequent. Three years ago was in hospital for *attack of indigestion* (Italics mine). Two years ago was in hospital twice; once for chronic bronchitis and again for *chronic gastritis*. Both times went home improved; appetite good; has gained flesh

in last two years. While in usual health, four days before entrance to hospital, began to have pain in region of the umbilicus, sharp and sickening in character, lasting only a few seconds and gradually wearing away. There were also nausea and vomiting. Attacks continued at varying intervals. Bowels moved up to the last day." In the light of the demonstrated cause, how significant the attacks of indigestion and chronic gastritis; how important the usual good health and the good appetite, and the fact that there had been no loss of flesh! As in this case, so, I dare say, in many another, the gallstones are reported as without symptoms, whereas the fact is that we are of insufficient acumen to recognize the symptoms, when, especially in the light of operative demonstration, they seem glaringly conspicuous.

In a few cases the fact that gallstones may exist for years with few if any symptoms, or with symptoms which are unnoticed, has led to an erroneous diagnosis of a gallstone cause for jaundice. The absence of gallstone pain and discomfort in histories must not receive too much weight in eliminating gallstones from the diagnosis; for, as I say, they may exist and cause serious anatomical lesions with few if any symptoms. Nor must they be positively diagnosticated in the presence of jaundice, for many cases are met with in which jaundice exists without a gallstone cause, and many, it is true, in which the jaundice is the only symptom of gallstones. In considering gallstones without symptoms, therefore, the possibility of gallstones must always be borne in mind if a lesion is suspected anywhere in the biliary tract or contiguous to it, whether the lesion has symptoms suggestive of gallstones or not. Gallstones without symptoms are found usually in the course of abdominal operations for other lesions, when a systematic examination is made of the abdominal contents. They are also found when some other and more probable diagnosis has been made—ulcer of the pylorus or duodenum,

for example, some lesion of the kidney, or even acute intestinal obstruction.

Scattered through my records of recent years are many cases which illustrate these statements. In most of them, however, the history is at fault. In the light of the operative demonstration, as I have said, the significance of apparently unimportant symptoms becomes apparent.

At an exploration of cancer of the ascending colon in a man of fifty-nine years (vol. lix, 117) the gall-bladder was found tightly packed with gallstones which had never been suspected. Review of the history taken in much detail shows the following expressions which might have been based upon the condition of the gall-bladder: "Miserable, heavy pain in the left renal region;" "a dull, grinding pain, —it is awful." A tumor was later discovered in the ascending colon, and to this was attributed the pain. There was, however, no intestinal obstruction at any time, and intestinal cancer without obstruction is usually painless. Left-sided pain in serious gallstone disease is not so very uncommon, and in this case it was probably caused by the gall-bladder lesion.

A woman of forty-four years (vol. xxxvi, p. 27) was operated on for removal of a large fibroid, and at this operation a stone was found completely filling the gall-bladder. Not a word in the history suggested gallstones. Later the patient had "cramps in pit of stomach and breast. No pain in right side. Have to be very careful what I eat, and cannot lie on right abdomen. I have a good deal of pain, but I have got used to it and do not mind anything about it." Over the liver was a raw blister. The patient declared that before the hysterectomy she had always been perfectly well except for "bilious attacks," on account of which she had to be very careful in her diet. The bilious attacks before operation and the obscure symptoms after, were probably owing to the condition of the gall-bladder.

A woman of forty-nine years (vol. lvii, p. 59), at an hyster-

ectomy for fibroid was found to have a number of gallstones. Forty stones were removed by cholecystostomy. On reviewing the history of the case I find: "For six months was treated for gastritis and persistent pain in the epigastrium; colic spells without jaundice. Tenderness in gall-bladder. Better examine under ether and explore gall-bladder and pylorus." In this case I find that there were very positive indications of gall-bladder disease, but in the presence of a bleeding fibroid the significance of a gall-bladder involvement was lost.

At operation for removal of double multilocular ovarian dremoid cysts in a woman of fifty-one years (vol. lxii, p. 159) we found the gall-bladder contracted upon one large stone. A review of the history shows only this statement: "She has a fair appetite, but considerable gas in the stomach." In this patient it may fairly be said that there were at no time any symptoms of gallstones, and yet "gas in the stomach" may have been her way of expressing a gallstone irritation.

A woman of thirty years (vol. lv, p. 62), operated upon for double salpingitis, "for a year had had pain low down in the back and in the right lower quadrant. Things seem to knot up like cramps." In this case there was nothing that could reasonably be attributed to gallstones, but a gallstone was felt impacted in the cystic near the common duct.

On inquiry I have been informed that "previous to her operation she had acute attacks of what was called acute indigestion, and several times she called a physician to get relief. She had not been troubled with this for at least two years prior to operation. Occasionally she was heard to complain of a gastric pain for a moment, but nothing of any consequence. This she never associated in any way with the trouble for which she underwent the operation. She had never complained of any attacks since."

From this patient's age, and from the records, no one would have had a suspicion of gallstones. Had she died of some acute disease, and had the stone been found at autopsy,

her case would have been classed as one of the numerous instances of gallstones without symptoms. Knowing exactly the size and situation of the stone, I have little doubt that it will eventually be passed into the duodenum. An operation performed for gallstone symptoms would very likely prove useless, for there is but one stone, and that, as I say, small enough to be passed in a single brief colic. This information shows that the "acute indigestion" was the missing evidence of gallstone irritation which, if we search carefully, we shall find in most if not in all cases.

In the case of a woman of sixty-three years, at the removal of a large ovarian cyst, I found a large, flabby gall-bladder containing many small stones. The abdominal symptoms had lasted six years. The only symptom referred to the epigastrium was nausea, of which the patient complained a good deal, and which was probably due to the pressure of the cyst.

At an abdominal hysterectomy for fibroid in a woman of sixty-six years (vol. lix, p. 62), a small gall-bladder was found contracted upon several small stones, one of which, the size of a pea, was situated in the cystic duct close to the common. Going over her history I find (in the writing of my medical assistant, Dr. F. W. Palfrey): "Is said to have a fibroid. Her only complaint is indigestion for two weeks; distress in stomach and vomiting. Bowels moved only with croton oil at the beginning of her trouble, two weeks ago; since then they have moved of themselves. She has no pain; no feeling of weight or pressure. Has noticed an enlargement of the abdomen. No bleeding from vagina. Appetite good until the past few days. No loss of weight. Thirty-eight years before had a 'typhoid dysentery' followed by 'consumption of the blood.'" Three months later (October 9, 1906) the patient reported to me "in splendid health. Looks extremely well; red cheeks. No pain from gallstones. Has had 'those vomiting spells.' She said, 'In the first place, I would feel as if my stomach refused to

take anything I would swallow. That would be all there was to it. Some days I would vomit everything I would take into the mouth. Night before last something happened: I had dinner at 4.30 P.M.; had supper at 8 P.M. or 9 P.M. and went to bed. Soon began to have nausea. There was no pain. I did not vomit. The attack lasted a short time, and next morning I was all right. Had never had any gallstone colic.'"

In this case, as no doubt in a great many others, the operation though plainly indicated, even demanded, was performed upon the less acute and less serious lesion. In the light of the operation, the nausea and indigestion had nothing to do with the fibroid. Though the growth was a large one, there was neither pain, pressure, nor hemorrhage, in fact it was causing no symptoms. But for my unwillingness to impose one capital operation upon another, I should have removed the gallstones.

These cases, taken mostly from the records of the past year, are by no means infrequent. In bringing them together I expected to show fewer signs of gallstones in the histories studied after the demonstration of stones. It is, I think, true that few gallstones exist which do not cause symptoms of some kind at some time or other. It is true, too, that these symptoms are so unusual, and so unlike our preconceived ideas, as to be of themselves either non-suggestive or wildly and absurdly so. I am seriously of the opinion, nevertheless, that any gastric or epigastric symptoms, persistent and unmistakable, recurring at irregular intervals or not recurring at all, are, in the presence of gallstones usually due to those stones; and that progress in the direction of accurate diagnosis demands long-continued observation of patients in whom stones have been demonstrated and left undisturbed.

Recently one of the younger men, to whom I referred a case of supposed gastritis far away in the country, made a diagnosis of acute cholecystitis, and advised operation on subsidence of the acute attack, or in case the symptoms were

growing worse. The patient suddenly and unexpectedly died of the perforation of the duodenum from unsuspected ulcer. A week or two later the same physician, in consultation with one of the younger men, a skilful and experienced surgeon in whom, as well as the physician, I have the greatest possible confidence, advised an operation for supposed gallstones. Present at the operation was an older physician who had scouted the idea of gallstones and had diagnosticated gastric ulcer. One can imagine the chagrin of the younger men at finding no gallstones when gallstones had been confidently expected. There was no gastric ulcer, but the bile was thick, dark, and tenacious; there were masses of mucus suspended in it. In a word, there was an abnormal condition of the gall-bladder inviting infection, causing colics, and, in my belief and experience, making the gall-bladder the nidus of future gallstones—a condition for which cholecystostomy and drainage are the best remedies. There was no cause whatever for chagrin. The only error in such cases is in limiting the cause of symptoms to an actual crystalline formation. This experience I have had several times. Furthermore, I have operated for gallstones and have not found even the changed bile or any other abnormal condition in the biliary tract.

A physician from Philadelphia (vol. lvii p. 167), thirty-nine years of age, six months after typhoid (in 1889) had his fitrs attack of colic. During two years the attacks were repeated every three months and were accompanied by jaundice. No stones were found in the stools. From that time to operation (November, 1905) he occasionally had jaundice induced apparently by fatigue, emotion, or indiscretion in diet. The colics ceased in 1891, but there has been ever since more or less pain in the region of the gall-bladder. The pain has rarely been radiating, but has been distinctly circumscribed in the gall-bladder region. The diagnosis was cholecystitis. The patient was in the hospital two or three months with jaundice and clay-colored stools. He had a "subacute

gastritis or duodenitis," and the temperature went as high as 104°. There was a good deal of gastro-intestinal fermentation with eructations. The diagnosis of gallstones was made by one of the best surgeons in America, upon whose diagnosis and recommendation I would willingly have operated even had I not concurred fully in his diagnosis. At operation I found the gall-bladder rather large and full of fluid. No stones were felt in the gall-bladder or cystic or common duct. Between the top and left side of the gall-bladder, running over to the stomach and constricting slightly the pylorus, were a number of old adhesions, probably of inflammatory origin. These adhesions were dissected away, so that the gall-bladder and pylorus were entirely freed.

I was much surprised to find no stones. The bile was distinctly changed; it was dark, dense, viscid, and opaque. On drainage it became thin, transparent, and yellow like normal bile. This patient was completely cured.

A girl of twenty-three years, from Honolulu (vol. lvix, p. 102), had been operated upon for appendicitis in 1904, just after an acute attack. There was said to have been sepsis, so that recovery was slow and imperfect. After the operation there was pain in the right side with occasional severe exacerbations. The pain was intermittent and hard to bear, recurring every month or so. It radiated from one spot and was accompanied by neither vomiting nor jaundice. After the pain there was tenderness in the gall-bladder. Gallstones were diagnosticated by one of the most experienced and skilful of the younger physicians in Boston. The diagnosis was based chiefly upon the fact that, after an acute attack, he found definite tenderness in the gall-bladder. At the operation the gall-bladder was found filled with dark green bile of greatly thickened consistency. The common and hepatic ducts were free of stones. The head of the pancreas seemed thicker than usual. By means of a tube the gall-bladder was drained for two or three weeks. Then

it was allowed spontaneously to close. The patient was completely relieved of her pain, and has been well ever since.

In this case the condition of the bile was such that its passage through the cystic duct was difficult. As in all such cases it ran very slowly through the small aspirating needle.

In another case, that of a woman of about thirty-five years (Operation Book, 1902, p. 213), the diagnosis of gallstones had been made from intermittent attacks of colic with subsequent tenderness over the gall-bladder. The diagnosis in this case was made by the husband who was a surgeon of experience. The bile was precisely like that found in the two preceding cases. Drainage for two or three weeks resulted in perfect and permanent cure.

The last case I have seen was that of a man of forty-eight years (vol. lxiv, p. 77), who for two or three years had had pain in the right hypochondrium, gradually increasing in severity. The last attack, three days before my examination, was described as a sudden onset of severe pain, like the stab of a knife, in the exact region of the gall-bladder. The pain radiated through the abdomen to the right shoulder. There was no jaundice. These attacks were irregular in their appearance. I found the gall-bladder perceptible and prominent and sent the patient to the Massachusetts General Hospital where my assistant operated for me. The gall-bladder was soft, non-adherent, and not distended. It contained no stones and was apparently normal in every way. The adjacent viscera were all normal to the touch. The gall-bladder was not opened. The patient made a good recovery and has had no pain since.

This is a very recent case; the patient has just been discharged from the hospital. I did not see this operation, but an absolute demonstration of the condition of the interior of the gall-bladder was not made. I have found in the gall-bladder gallstones which I could not feel before the gall-bladder was opened. I have had the gall-bladder out of the

body, between my fingers, and then have been unable to feel a gallstone inside.

The final case was that of a woman of about fifty years, upon whom I have just operated for the closure of a large omental hernia, following an operation for supposed gallstones. This patient had the symptoms of gallstones, but at the first operation, which was performed by an assistant at the Massachusetts General Hospital, none were found. The head of the pancreas was thickened as in the cases of supposed pancreatitis. The patient was entirely cured by the operation and came back at the end of a year for closure of the hernia.

Scattered through my records in the past ten years are several cases of this kind. I am convinced that in many instances the real lesion is one of the gall-bladder, with such changes in its contents that they are emptied through the cystic duct only with increased difficulty. This results in a painful spasm, just as the passage of a stone through the duct does. It is a matter of conjecture whether this condition of the bile is one which eventually produces gallstones. I have sometimes found, in bile thus changed, small, soft stones, apparently of recent formation. The chief symptom in these cases is pain—the gall-bladder contracts with such violence as to produce a sudden severe spasm. The pathological changes may be slight or extensive, without gallstones or with them. The inference must not be too hastily drawn, that because no actual crystalline masses are found in the gall-bladder, that viscus and its contents are healthy. I should not regard a gall-bladder as demonstrated to be normal unless the interior of the gall-bladder itself had been actually examined and the condition of its contents accurately demonstrated.

There remains much for us to learn as to the effect of gallstones in the gall-bladder, especially in those early stages when the stones have hitherto been regarded as innocuous and without symptoms. In going over my his-

tories, in the light of the demonstrated lesion, I find it difficult in any case to establish an indisputable history of inoffense. So common are gallstones, especially in women beyond the menopause, that it seems to me wise in all abdominal cases, no matter what the diagnosis, to question carefully with reference to obscure abdominal symptoms, especially those connected with digestion. Not only may the surgeon predict the presence of gallstones in patients operated upon for something entirely different, perhaps for a lesion in the pelvis, but he may discuss with the patient and her friends the desirability of removing such a menace to health whenever the additional operation would add but little to the risk. In proposing an operation for the removal of a fibroid, for example, one might, by the detection in the history of obscure epigastric symptoms, be prepared to relieve some abnormal condition in that region. I have found not only changes in the biliary tract, but many variations from the normal in the stomachs of women operated upon for pelvic diseases. I have not felt at liberty, without consulting the patient, to add the burden of operation upon the stomach, upon the gall-bladder, or upon the appendix. In future I shall make as careful inquiry into digestive disturbances as into the condition of the heart, lungs, and kidneys. Not that I have not always noted the appetite and the processes of nutrition, but, as in one of the cases already reported, previous attacks of indigestion and colic often are not mentioned by the patient, because she thinks that they are in no way connected with her complaint, which in the case mentioned was salpingitis.

I have sometimes felt, of recent years, that in writing much upon the surgery of the biliary tract I have been perhaps open to the criticism of telling a twice-told tale—of threshing out old straw, but still I must insist that for me, at least, the right upper quadrant is a field far from being fully and satisfactorily explored and understood. Every day I see conditions for which, in my own experience and in the

literature of the subject, there is no precedent. The two conditions which I occasionally meet with, comprised in the title of this paper, present questions which demand much more experience than I, for one, am able to give for their perfect solution, which can be settled only by renewed study and abundant discussion.

# CHOLECYSTECTOMY, WHEN INDICATED; SOME OPERATIVE SEQUELÆ.

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A few years ago gallstone surgery was rarely done, and it was reserved for Lawson Tait to make it popular. Since then it has been the effort of surgeons generally to attempt measures looking toward the prevention of some of the poor results of gallstone surgery, and hence the operation known as cholecystectomy grew in favor because one felt sure of preventing any future trouble such as fistula, cancer, and the prevention of reformation of calculi, etc.

At first the operation rapidly grew in favor, but very soon it was discovered that it too had its limitations, and it is our object in this paper to show that, however easy the operation, there are good reasons for leaving the gall-bladder except when it is incapacitated by disease. The experience of urgeons who have had a large opportunity for the investigation and treatment of the diseases of the biliary system should be interesting. The credit for the first cholecystectomy is accorded to Langenbuch,[1] who operated July 15, 1882. But we find that in, 1829, Campaignac published a paper entitled, "Des plaies des voies biliaries, de la ligature du col. de la vesicule, et de la ligature partielle de cet organe." *Jour. hebdom. de med.*, 1829, ii, p. 204.

We have selected the following rules laid down by Moynihan[2] as a guide in the selection of cases for cholecystectomy, for we believe them very nearly representative of the best thought of the surgical world of today:

"1. For injuries. 2. Gangrene. 3. Phlegmonous cholangitis. 4. Membranous cholangitis. 5. Chronic cholecystitis. 6. In distention of the gall-bladder." In this heading Moynihan includes 'hydrops, or empyema due to blocking of the cystic duct by calculus, stricture, growth of external inflammatory deposits, or mucous fistula following operations for these conditions.' 7. In cases of fistula between the gall-bladder or cystic duct on the one hand, and duodenum, stomach, or colon on the other. 8. In multiple ulceration of the gall-bladder or cystic duct where stones have eroded their way through its walls into the liver, the duodenum, or other protective adherent masses. 9. In primary carcinoma of the gall-bladder." The above rules do not sanction an indiscriminate onslaught upon the gall-bladder. Moynihan[3] further says (*Gallstones and their Surgical Treatment*, p. 326): "The gall-bladder should be known as useless before cholecystectomy, and its removal should not add risk as compared with cholecystotomy." In the abstract one might be inclined to think that the loss of a bile reservoir capable of emptying on demand would be a serious matter to the individual, or at least a disability. "The perfection of the mechanism of digestion (Parlow) would require that the bile should be ejected in spurts, as it were, into the duodenum during digestion. But there is clinical experience in abundance to show that when all of the bile is discharged from the body through an external biliary fistula, without a drop entering the intestine, the individual suffers no sign of disability of any kind." He is, moreover, inclined to a greater proportion of 'ectomies, but he calls attention to the greater difficulty of a second operation if stones should re-form in any one of the ducts, after an 'ectomy.

Mr. A. E. Barker,[4] a London surgeon, proposes cholecystectomy in those cases requiring choledochotomy and a search of the ducts for stones. Also, for simple ordinary cholelithiasis. It is evident that this author practically

uses the operation as a routine procedure, but he goes on to specify that it is necessary in suppurative lithiasis, or acute infection of the bacillus coli, or when there are adhesions, or for gangrene of the gall-bladder, or for malignancy. We have here a type of radical treatment which demands careful thought, for this surgeon violates almost all precedents in that he even operates in many infectious conditions without providing for drainage of the hepatic ducts, as recommended by Kehr and others.

Dr. Martin,[3] of the University of Maryland, reports six cases of cholecystectomy occurring among twenty-seven gall-bladder operations. Dr. Martin well says that we have prompt immediate recoveries after 'ectomy, and that there can be no future gallstones in the gall-bladder, or further attacks of cholangitis, etc. This author is fortunate in his results and is looking with great favor upon this operation, although he is draining most of his infectious cases by way of the gall-bladder. In a very excellent review of gallstone surgery, Wathen[6] says 'ectomy is indicated in contracted and cicatricial bladders, or in malignant growths, or in contractions of the cystic duct, which prevent its physiological purpose, or in phlegmonous cholangitis.

McLaren[9] proposes cholecystectomy and choledochotomy at the same sitting, and dilates and drains through the stump of the cystic duct. He favors 'ectomy in perforations, but says the operation should be done in those cases where the infection is not localized. He opposes 'ectomy in gangrene or in the presence of a localized abscess. Huntington[8] removes the gall-bladder in traumatic cases where immediate suture is impossible, or when there is stricture of the cystic duct, or in gangrene or malignant disease. He says the danger of overlooking a stone is a good reason for leaving the bladder at a first operation. (We note this as opposing Martin's suggestion that the bladder be removed for the same reason.) McLaren has had his troubles like many others, as for instance when a patient con-

tinued ill for a long time after operation, until a stone passed, with final recovery. He also quotes Mayo, who operated successfully for persistent biliary fistula after 'ectomy, by making an anastomosis between the hepatic duct and the duodenum. He has been more conservative since then, and says it is discouraging to have a patient develop an obstruction of the common duct after cholecystectomy.*

Several surgeons perform 'ectomy during attacks of acute infection, and Erdman[10] compares the operation with appendicectomy. He, like Moynihan, says we can live without gall-bladders, and if they are the seat of disease which time cannot cure, or when they are a menace to life, they should be removed. One of Erdman's patients had a return of pain after 'ectomy with hepaticotomy and choledochotomy. Fortunately, this attack did not necessitate operation. Two deaths are reported by this author, but these were not in infectious cases. We agree with Erdman that tolerance ought to be, and we add *will* be, established when a contracted bladder has existed for a long time. That is to say, that the function of the gall-bladder should be assumed by the other channels, such as the hepatic and common ducts.

As we all know the Mayo brothers are nearly represented in the views we have already expressed as the opinion of other operators. But they stand for certain distinctive opinions which are the outcome of an immense experience which is perhaps unrivalled. Besides having an opportunity to critically examine many gall-bladders which they have removed, they have at great pains and labor made an investigation of the relation of gallstones to cancer, which occurs in 5 per cent. of their cases.

Unlike Professor Kehr, they do not think stones often reform in the gall-bladder. They frequently drain the hepatic

* Baldy reports an interesting case of jaundice with pain and recurrence of all the former symptoms after 'ectomy, which several after operations were unsuccessful in relieving.

ducts after a cholecystectomy, and they accept Davis' teaching as to proper drainage after opening the common duct. Briefly stated, they remove the gall-bladder when it contains mucous, or when the cystic duct is closed, or the seat of disease which may render it impervious to the passage of bile. Mayo's indications are: 1. When there are stones lodged in the cystic duct, removal of which might be expected to leave a permanent interference with drainage. 2. When the disease is confined to the gall-bladder and the gall-bladder is filled with pus, or cystic fluid, or bile, which has undergone septic change. 3. When the gall-bladder has become greatly thickened and lost its function. 4. Cholecystitis without stone and in which the liver ducts are not involved. 5. Malignant disease. Perhaps their most distinctive technique in this operation is the enucleation of the mucosa of the bladder without taking away the peritoneal coat. We have tried this operation and we find it as they say quite easy to separate the two coats near the lower end of the gall-bladder, and we then tie the duct and then cut away the bladder, and after completing the 'ectomy we secure a tube in the stump of the bladder and hold it there by a purse-string closure of the outer coat.

In common-duct cases the gall-bladder is not removed except for good indications, as there might be necessity for a second operation in which the gall-bladder might be a good guide to the deep ducts, and, if necessary, a cholecystenterostomy could be done. In normal uncomplicated cases cholecystectomy can be done, but the mortality is a little higher, or 1.64 per cent., while cholecystotomy gives 1.37 per cent. They find indications for removal of the gall-bladder on an average of two out of five cases.

Of all surgeons, we find Mayo, Robson,[11] and M. Richardson the most conservative. Robson has seen gallstones develop in the common and hepatic ducts after 'ectomy. He knows from his own experience that in some cases the hepatic and common ducts dilate after 'ectomy, and that stones form in

them. Moreover, he asks a very serious question, "May not cirrhosis of the liver or pancreas result from cholecystectomy?" He removes useless bladders, but only when he has ascertained the patency of the ducts, and absolutely opposes the removal of the bladder in ordinary cholelithiasis, or when it may be used for drainage purposes as in chronic interstitial pancreatitis. In 57 operations, 33 were for gallstones complicated by various morbid states, of which 32 recovered; 13 were for mucous or other fistulæ.

Dr. Maurice H. Richardson[12] opposes a routine performance of cholecystectomy. He removes the gall-bladder only in rare instances, but we notice that he is performing the operation more frequently. Dr. Carr,[13] of Washington, takes a similar view. Dr. Charles R. Robins[14] read a most excellent and comprehensive paper before the last session of the Medical Society of Virginia, in which he advocates the operation because of the quick recoveries, absence of drainage, and because stones do cause a permanent injury to the bladder, and because stones are left hehind in cholecystolithotomy. He favors the retention of the viscus in cases where the bladder is healthy and where there is acute cholecystitis and in chronic interstitial pancreatitis. In obstruction of the common duct he favors its retention because it offers the only way to provide an exit for the bile into the intestine by means of a cholecystenterostomy.

Professor Kehr[15] has recently published a book telling of his experience in 1000 cases of gallstone disease. He says of cholecystectomy, "It is the ideal operation" and that "when a bladder is repeatedly inflamed it becomes incapable of fulfilling its functions, and then becomes the chief place in which stones can form, making its extirpation most desirable." He, however, counsels great care (p. 193) in the selection of suitable cases for operation and advises its retention in those cases associated with pancreatitis, or when the bladder is needed for cholecystenterostomy. He

opposes resection of the bladder or extirpation of the mucosa (Mayo's operation). He calls resection a half operation, and does not favor any of the modifications, such as Lindner's or Kottmann's. He says, "Not only should the gall-bladder be taken out, but not a remnant should be left to induce future inflammation." (p. 204.)

Professor Kehr stands for what he calls "hepaticus drainage," and the 'ectomy is a preliminary to this feature of his work.

In looking over Professor Kehr's statistics we find that very seldom does he have to deal with a recurrence. (*Vide* p. 409, also 419.)

Riedel[18] in gallstone surgery is also notable, although his statistics of cholecystectomy are not very full. He proposes 'ectomy in three instances, as follows: 1. In obliteration of the cystic duct with small gall-bladder. 2. In choledochotomy with open cystic duct, in certain cases. 3. In persistent biliary fistulæ, mucous or otherwise, or after cholecystotomy which has left an obstruction or obliteration of the cystic duct.* Riedel collected 72 cystectomies with a mortality of 16. In 7 of these death was immediately due to the operation.

### *Some of the Complications of Cholecystectomy.*

These are comprised under two distinct heads. One of these has reference to the difficulties of the operation itself, such as may result from local infection, as after rupture of

* We are indebted to Calot for an exhaustive paper on cholecystectomy, which was published in 1890, and we find the whole subject well handled except in one particular: he reports no accident or sequelæ which would deter one from thoughtless use of the operation. All of the cases he reports from the literature have been prompt cures. In this paper we have avoided all but comparatively recent reports and have tried to give a correct idea of the present status of the operation of cholecystectomy.

the gall-bladder, and the other has to do with certain results which occur after 'ectomy, which are now known to follow in the wake of the operation. Let us briefly consider the propriety of the operation itself as an operation of choice.

We have already quoted a sufficient number of authorities to show that a bladder which is innocent of any infectious or malignant disease may be allowed to remain if the cystic duct is open. Almost all of the authors quoted favor this view. There is nearly unified sentiment favorable to 'ectomy in injured and ulcerated gall-bladders, especially when the cystic duct is occluded, or in empyema, hydrops, etc. All authorities are in harmony on the treatment of pancreatitis by drainage through the gall-bladder, although there is some difference of sentiment about the wisdom of cholecystectomy when the hepatic ducts are involved. We are of the opinion that very many reports of post-operative mischief will ultimately be brought to light and the proper estimate be given the operation. It is to us a matter of surprise that so few reports where patients have had trouble after cholecystectomy are to be found in the literature. The following cases have come under my own observation:

Mr. R., was operated upon in the hospital of the University of Georgetown, for cholelithiasis with cholangitis on July 20, 1905. An unsuspected but very mild pancreatitis was present and a mucous fistula resulted with necrosis of the abdominal scar. After six months we again operated for persistent fistula, and removed the gall-bladder, thinking to effectually dispose of the case. We carefully explored the ducts and satisfied ourselves that the pancreas was healthy. We used a double strand of No. 2 chromic catgut in ligating the cystic duct, and sutured a drainage tube to the peritoneal cover of the stump. On the fifth day after operation, bile began to pour out of the wound. This continued for many weeks before the wound finally

closed.* This patient had no further pain after 'ectomy, and his condition steadily improved in spite of the free discharge of bile for such a great length of time.

A second case occurred during the present year. Mrs. P., a patient in Columbia Hospital, was operated upon for gall-stones with contracted bladder, and with associated jaundice. Diagnosis at the time was common-duct obstruction, as the patient had previously been severely jaundiced. We removed the gall-bladder which was full of stones, and also one stone from the common duct near the cystic duct. The immediate result was perfect, but at the end of three weeks the patient had a sharp attack of pain and passed a small stone. Since then she has been passing stones at intervals of a few weeks. Three or four of these have been found, and are now presented for inspection. They are entirely unlike those found in the gall-bladder, which were large and of different color. These have, without doubt, passed down from the hepatic duct, and we are hoping that each attack may be the last. Another patient (not my own), the wife of a professional man, has had cholecystectomy for cholelithiasis with jaundice and history of repeated attacks of pain. Besides the stones found in the bladder several were found in the hepatic duct, and they continued to pass out through the drainage tract for a time after operation. Finally, the patient appeared to have recovered and the wound closed. But the end was not yet, for the patient has continued to pass stones or to have attacks of pain associated with jaundice from time to time, during nearly two years since operation. This patient was operated upon by a most competent man whose reputation is unsurpassed for technical skill.

Conclusion.—It appears to us that the operation of cholecystectomy is doomed to fall into ill favor unless we can

* I am informed by my friend Dr. Ready that this patient did have severe pain after all the stones had been removed from the gall-bladder, showing that another stone had passed from the hepatic duct, which had been overlooked at the operation.

satisfy ourselves that stones are not left behind in the hepatic ducts. We, moreover, are inclined to the opinion that under certain conditions the bile is more readily made to precipitate cholesterine and consequently stones may form more rapidly and in greater numbers than we have hitherto been led to believe possible from results of experimental work. We think there is good reason to believe in the existence of "gallstone disease" aside from infection. Surgeons of average or more than average ability are having positive proof of these after-results of operations, which are, from the present view-point, rather discouraging. As an alternative we are considering with greater favor the teaching of Professor Kehr, namely, that more cases require examination and perhaps washing of the hepatic ducts. Surely, the demand of the present hour is for a safe and certain method of insuring the patient against future attacks of pain after gallstone operations. It may be well to take into account the possibility of help from our internist friends, who may yet find some way by which the formation of gallstones can be prevented.

We have seen poor results of our cholecystolithotomies and have resorted to cholecystectomy to insure against a recurrence of pain only to be occasionally disappointed after a more radical operation.

REFERENCES.

1. Langenbeck. Ein Fall von Extirpation der Gallenblase, wegen Chronische Lithiasis. Heilung. Berl. klinischer Woch., 1882, No. 48, p. 725.

2. Moynihan. Brit. Med. Jour., London, 1903, vol. i, p. 186.

3. Moynihan. Book, p. 326.

4. A. E. Barker. Brit. Med. Jour., London; Clinical Jour., London, September 26, 1906.

5. F. Martin. Med. News, New York, 1905, lxxxvi, p. 1113.

6. Wathen. Surg., Gyn., and Obst., Chicago, 1906, iii, p. 177.

7. McLauren. Perforation of the Gall-bladder, Trans. Amer. Surg. Assoc., Philadelphia, 1905, xxiii, p. 207.

8. W. Huntington. California State Med. Jour., 1906, iv, p. 59.

9. Ladinski. Med. Record, New York, July to December, 1904, vol. lxvi, p. 995.

10. J. Erdman. New York Med. Jour., New York, 1906, lxxxiii, p. 379.
11. A. W. M. Robson. Brit. Med. Jour., London, 1903, p 183.
12. M. Richardeon. Med. News, New York, 1903, May 2.
13. W. P. Carr. New York Med. Jour., New York, Feb. 24, 1906, p. 384.
14. Chas. Robins. Virginia Med. Soc., February, 1906.
15. Hans Kehr. One Thousand Operations for Gallstones, 1905.
16. W. J. Mayo. Annals of Surgery, Philadelphia, 1902, vol. xxxv, January to June.
17. W. J. Mayo. Med. Record, New York, February 21, 1903.
18. Riedel. Handbuch der Therapie inn. Krankheiten, etc., Jena, 1898, iv, 2 Aufl.
19. F. Calot. De la Cholecystectomie, Thése, Paris, 1891, 211, p. 40, No. 52.

## DISCUSSION ON PAPERS OF DRS. RICHARDSON AND STONE.

Dr. Robert T. Morris, of New York City.—Allow me to speak of Dr. Richardson's paper. There is a very important reason why we have not had this subject brought more sharply before us, namely, we have done all the talking in cases in which we have found gallstones, and the patients' families have done all the talking in cases in which we have not found gallstones. In the past we have groped over the abdominal wall in the dark. We have since touched the button and turned on the light. I believe we have turned on the light upon this subject, or will do so if we include all gallstone cases and cholecystitis cases under one heading and call them cholecystitis cases, adhesion cases, or, as I called them last year, "gall-spider cases."

Consultations were held years ago to decide whether an appendix had something in it or not; if it did not have anything in it we were not to operate. The same question comes up with reference to the gall-bladder. Are there gallstones? Yes; operate. If there are no gallstones, do not operate. That leaves us in gall-bladder cases right where we were twenty years ago in appendicitis.

Now, adhesions in the region of the gall-bladder and bile ducts will produce the same symptoms that we get in cholecystitis whether gallstones are present or not. Adhesions angulate the gall-bladder and the ducts. We should be talking

about adhesions now instead of stones; the stones are incidental and of no account; it makes no difference whether we find them or not; they have little or no bearing whenever I propose operation in a case of that sort nowadays. Even from that position I still make mistakes.

A man, very well situated financially, was sent to me from Helena, Montana, two or three years ago, with a diagnosis of gallstones having been made. After examining him carefully I said: "I cannot prove that you have gallstones or adhesions." He went to Dr. Mayo who removed gallstones. (Laughter.) He was a nice patient too. (Laughter.) That man wanted relief, and came to me, and I did not give it to him.

Another man came to me from Montgomery, Alabama, some time ago, with a diagnosis of gallstones. I made a diagnosis of gallstones. I operated but did not find any stones; he had a lot of adhesions. I operated on this man three weeks ago.

There is a constant infection going on in this region. The bacteria are taken up from the bowel wall from the afferent vessels, are carried to the liver, are excreted, and cause infection on the way out. The gall-bladder is one of the remnants which is not able to protect itself. Like the wisdom tooth and the appendix it is capable of making a lot of trouble for us, and a gall-bladder with thin walls does not protect itself. Constant infections are going on from childhood up from the bowel bacteria, which are carried to the liver and out, causing a lot of low-grade inflammation in this vicinity in a very large proportion of all people. Formerly, we called these cases gastritis, malaria, etc., the same as we had different wrong names to apply to appendicitis. But never mind, we have a condition going on there, so that in the dead-house it is hard to find a subject with a free gall-bladder. The way to classify these cases, I believe, is to talk about them as adhesion cases. I believe that we should operate for adhesions, separating the adhesions to relieve symptoms, and consider the presence or absence of stones as incidental.

With reference to the paper read by Dr. Stone, I am becoming more and more inclined toward cholecystectomy. The proper selection of cases is everything in surgery; but, in brief, my tendency is more and more toward the complete removal of the gall-bladder for the reason that we have a chronic infection going on all the time in so many patients. After

you have removed the gallstones, if you have infection continuing, you will have lots of trouble from adhesions. When we come to the complete removal of the gall-bladder, it is largely a question of technique. I find that my technique is getting better all the while, and it seems to me we can remove the gall-bladder as we remove the appendix in about the same way and about as easily and safely. That is my idea at present.

Dr. W. P. Carr, of Washington, D. C.—The preliminary remarks of Dr. Morris are very applicable. We ought not to think so much about gallstones, but should consider these cases more as instances of cholecystitis and regard the gallstones as incidental. When we remember the fact that gallstones may not cause any symptoms whatever of themselves until they produce cholecystitis or obstruction of the common bile ducts, it is a matter of impossibility to diagnose gallstones until these conditions come on, and it is really the cholecystitis or the obstruction of the bile ducts that leads to the diagnosis of stones. A great many other things can cause cholecystitis and obstruction of the bile ducts; consequently, it is impossible to make a diagnosis of gallstones with accuracy. I have had a number of cases in which there was obstruction of the bile ducts, but in which we could not determine the cause until after operation. It is impossible to tell whether or not a patient is going to get some obstruction that cannot be removed and who may need another operation later for draining the bile.

I think for that reason the gall-bladder should be left unless there is a positive indication for its removal. The explanation for some of these obstructions being relieved by simple drainage is the fact that the gall-bladder is against the abdominal wall, at its upper end, and cannot expand in that direction, and when inflammation occurs which causes a temporary occlusion of the bile ducts it begins to distend, push downward, and drag the ducts with it so as to cause a kink in the ducts, and then the tighter the gall-bladder becomes the more positive this valve-like kink is in its action. Just as soon as we empty the gall-bladder and relieve tension the duct straightens itself, and obstruction if present is relieved. We have all seen cases of obstruction relieved as soon as the gall-bladder was opened. It may be that the obstruction is caused by some stones down in the common

duct under the head of the pancreas, or by one of these kinks, which it is difficult to tell. If the gall-bladder is removed and it turns out to be a stone in the common duct, we may have to operate again. If we leave the gall-bladder and have good drainage, the stones may come out through the gall-bladder by working up. When the cystic duct is absolutely occluded there is no reason for leaving the gall-bladder, and where there is any suspicion of malignant disease it ought always to be removed.

I can hardly agree with Dr. Stone that patients may have biliary fistula and loss of bile without any grave symptoms. In dogs a biliary fistula always produces great impairment or deterioration of health, but it takes a long time to do it. The dog may not show serious symptoms for a year; a man may have his bile thrown out for a month or two without serious symptoms or consequences, but if this should keep up for a year or more the consequence would be very serious.

Dr. Samuel Lloyd, of New York City.—In reference to the case reported of occlusion of the gall-bladder and the non-escape of bile into the duodenum, there are two possible explanations. The occlusion of the duodenal meatus in these cases of gallstones is not infrequent. I have operated once on a case of congenital closure of the opening from the ampulla of Vater into the duodenum, and we will find in some of the cases of gallstones a very much distended common duct; one which is distinctly larger than usual. In those cases we should suspect occlusion at the ampulla of Vater. The stone coming down and acting as a ball valve, pushing up against the duodenal opening, produces an inflammatory condition, and finally contraction. In these cases the only hope of possible recovery of the patient is from a choledochoduodenostomy.

There is one other condition that has not been mentioned and that is the inspissation of bile throughout the ducts. A good many years ago in operating with the late Dr. A. Palmer Dudley, a former member of this society, I saw one of these cases for the first time. It was a case in which we supposed we had gallstones, but were surprised to find the gall-bladder, the gall ducts, and the hepatic ducts filled with putty-like bile, and it was necessary to remove all of that material before we could get the bile to flow naturally. I have found that condition several times since in operating

on the common ducts, and by passing a spoon in the common duct I have been able to clean that material out and re-establish the flow of bile into the duodenum. It seems to me, that is a thing that should be considered in all those cases where we get a much dilated common duct.

So far as cholecystectomy is concerned, my own tendency is largely in that direction. When we have gallstone infection, or rather a gallstone condition with infection, the proper thing in my judgment is to get rid of the infected area as soon as possible, and the gall-bladder at that time is the infected area, consequently it should be removed.

Dr. Henry O. Marcy, of Boston.—I quite agree with Dr. Richardson that we should discuss this subject with a great deal of earnestness, and I am sorry he has been so modest in his bringing this question before us again in this way.

In the past I published a good deal of my work on this subject, but regret I have not the data in hand for you today. But in reference to the operation that has been mentioned I like you to feel that we should give credit to American surgery, for it was Dr. John S. Bobbs,[1] of Indianapolis, Indiana, a man of considerable repute, who, in 1864, opened the gall-bladder and drained it. J. Marion Sims was the first American to operate clearly and definitely for this condition, only once in 1878, in Paris, and why he did not operate again is entirely unknown to us. But those of you who will take the time and trouble to look up the subject will find that this master mind went over the ground as thoughtfully and carefully as any of us could today.

A Dr. Palmer, who came under my observation many years ago, called my attention to the fact that he had a large gallstone and he said, "Dr. Marcy, if you do not remove it now, the day is coming when you or somebody else will, and I hope you will promise me to remove it after death if not before." I have that gallstone now.

The first case I wanted to operate on was one in which three of our leading men were consultants. Two decided against operation; one declared there was malignant disease, while the other said I had no more right to operate for gallstones than to operate on the stomach. Dr. Bowditch declared that operation was feasible and ought to be done. Then the friends enquired, "Dr. Marcy, did you ever operate on such

[1] Born 1809; died 1870. Surgeon in Cival War, etc.

a case?" "No." "Is there any one in Boston who has ever operated?" "No." "Is there any one in New York who has ever operated for gallstones?" "No." One then remarked, "I would rather trust God than an incompetent surgeon." Therefore, I made an autopsy a few days later. It so happened that a little later this experience was duplicated. Then I determined that if patients refused operation when I proposed or advised it, I had the right to decline further care. I operated upon Dr. Wooster, of Vermont, in July, 1887. He was a man of exceptional intelligence and had clearly diagnosed his condition. This proved to be the first case designedly operated upon in America, since Dr. Bobbs thought he had a tumor of the ovary. In October, 1889, I removed a gallstone from the common duct, and, as it now appears, this is the first case on record. I operated on that same woman a few weeks ago and recently sent her home from the hospital. What was the occasion of the second operation? That enforces the remarks of Dr. Morris with reference to adhesions. She had a series of adhesions which had bound down the parts in such a way as to cause a biliary obstruction. She has recovered from the operation; what will be the subsequent history I do not know. I did not find recurrent gallstones; the gall-bladder has practically atrophied, almost as if it had been removed. We closed the incision without drainage.

In reference to draining the gall-bladder and the constant passage of bile externally, within three years I have operated on a case where there was absolute closure of the common duct and drained. For one year that woman had bile passing externally through the abdominal wound; she was comparatively comfortable. I opened the gall-bladder and united it to the intestine, which was followed by an easy recovery. This case shows that, at least, for twelve months bile had passed externally through the abdominal wound without serious damage to the system.

In reference to many of the cases that are overlooked—the surgeon should be emphatic in his relations to the general practitioner. Some time ago I was caring for a man of middle life who had a clear history of biliary obstruction and advised operation. He suddenly had temporary relief and said, "Doctor, I will wait until next time." A few weeks later, driving by the house, for he was an old friend

as well as patient, I saw crape on the door. The certificate of cause of death was given as heart failure by the physician called in emergency. I made an autopsy and found that he had a perforated gall-bladder, with gallstones in the gall-bladder, and general peritonitis. The pathological condition in that case would have been overlooked by a great many practitioners. The sum total of united experiences in this work is very valuable.

Dr. J. Montgomery Baldy, of Philadelphia.—There cannot be much difference of opinion in regard to the classification of the cases spoken of in this discussion. Dr. Richardson has referred to a vital point, one which, I am certain, has been corroborated by my own experience, namely: we diagnose a case as gallstones, and yet at the operation find no disease of the gall-bladder or gallstones. We may find none of the adhesions that have been spoken of by Dr. Morris. We find no cancer, no occlusion of the gall-bladder, and yet the patient has had undoubted symptoms, so far as we can tell, of longstanding gallstone trouble. On the other hand, we operate and find distinct evidences of disease of gross character; we apparently relieve the patient; he leaves the hospital, but sooner or later returns with the same symptoms. This he may do even after a secondary operation and cholecystectomy. I have in mind one case of long standing of that character.

I must admit, that the whole field as yet is very largely unsolved, so far as my practice is concerned. There are elements of doubt both diagnostically and pathologically. These cases are generally all considered as infectious because they have gallstones, and yet I removed gallstones from the cystic duct of one case which was absolutely occluded and had been so for years without the patient manifesting any symptoms. There was an enormous stone in the duct but no infection in the gall-bladder.

It does not appeal to me as reasonable that removal of the gall-bladder will reach the seat of the disease which is causing the trouble and will cure the patient in such a case. I find that other men as well as myself have had unsatisfactory results, which make us doubt very much that we have come to a final knowledge in this class of disease. A gallstone or gallstones may be a minor factor in the trouble; they may be a result of the trouble, but I do not believe in the vast majority of cases they are the cause, nor do I believe they are

all caused by infections. Cholecystectomy in actual disease of the gall-bladder, where the gall-bladder is thickened, its walls destroyed, and there are adhesions, is indicated. There can be no question as to its removal in such a case. In a large number of cases I have obtained by drainage as satisfactory results as I have ever secured from cholecystectomy, and only in a small group of cases do I consider cholecystectomy indicated or called for.

Dr. Joseph Price, of Philadelphia.—The globe-trotting operators have not done good work in Philadelphia. They have been turned down in this city (Baltimore) and in Chicago. Kehr came to Philadelphia; he operated on four patients for gallstones and gallstone disease, and they all died. I received a message to come and see one of the patients he had operated on—a fifth case—who was dying from obstruction of the bowel. I was called to Norristown to operate on this man for the relief of his bowel obstruction. I found a condition of affairs that I had not witnessed before. His pylorus was anchored to the anterior abdominal wall. I relieved the intestinal obstruction and the patient recovered.

With reference to drainage, it strikes me that we are draining too much. That may impress you as a queer statement coming from me, because I have been criticized severely for draining—draining everything. But early in my experience in gallstone surgery I did not drain and I had good results. I had no complaints. I had pleasant results; I had satisfactory cures. I closed them all; I did not drain. Later I drained about everything and my results were worse; they were less satisfactory, altogether so, and in the last 16 cases I have not drained. But while doing this work there were 4 cases of rupture of the gall-bladder. The patients had carried the gallstones as long as the gall-bladders would carry them, and consequently there were four ruptures. In two of the cases, when I opened the abdomen, I had to side-step to get out of the way of the filth of the gallstones. These patients were neglected, and I was called to see them when they were dying. They were not suitable cases for the removal of the gall-bladder. I should have lost four of them on the table had I attempted to remove the gall-bladder. I made toilets and drained; I put in coffer-dams. These patients are all well, without any of the symptoms which commonly follow drainage. That had much to do with assisting drainage.

A year or more ago I operated on a doctor in Chambersburg for suppurating gall-bladder. When I incised the gall-bladder, which was as large as a California pear, I side-stepped to get out of the way of the gush of pus. This doctor has made a nice recovery; but he has a sinus, and when I saw him last I thought it had closed. I said to him, "I am going to make a closure of the gall-bladder." Later he got out of bed, put his clothes on and went home. I do not know whether any other surgeon has removed the gall-bladder since or not.

I do not recall but one patient upon whom I operated for gallstones and found none.

With regard to the early history of this work, Dr. Marcy did not give us quite all of it. If I remember correctly, Bobbs found gallstones; Sims followed Bobbs; then Musser and Keen. Musser and Keen took up the surgical aspects of gall-stone trouble and did good work.

At a meeting of one of the Southwestern medical societies, not long since, a practitioner spoke to me about operating on gallstone cases and asked whether I had done many such operations, and I said to him, "That is an operation we do before breakfast and say nothing about it."

Dr. Frank Martin, of Baltimore.—Dr. Stone has called attention to a paper I published in the *Medical News* in 1895, in which I reported 2 cases of cholecystectomy. I have not changed my opinion since then, although I have had but a few cases since. I have done cholecystectomy in 14 out of 60 odd cases.

I agree with Dr. Richardson that in the vast majority of cases of cholecystitis, whether there are gallstones present or not, biliary drainage is the thing we seek for and I get better results from it. But there are a few conditions in which we are warranted in doing a cholecystectomy, and among them are the chronic gall-bladders which have to come out. In most of the 14 cases there was a chronic cholecystitis, with disorganized gall-bladders and acute gangrenous infection. In two or three instances there was acute gangrenous inflammation of the gall-bladder. In those cholecystectomy has been resorted to with a good result. There has been no death and no serious shock attending the operation. I do not regard cholecystectomy as a serious operation.

I agree with those who have spoken that biliary drainage is the best procedure in the vast majority of cases.

Dr. Howard A. Kelly, of Baltimore.—I published a paper some time ago in reference to the importance of examining the other organs than those about to be operated on, after opening the abdomen (see *Med. News*, 1899, vol. lxxv, p. 784). Since that time I have also invariably put on my records the condition of this and each important abdominal organ, including the gall-bladder.

I almost always find definite symptoms where there are gallstones. Several times, when I have not gone into the history of my cases as carefully as I should have done, my head nurse has said to me, "Doctor, don't you think there are gallstones?" and I have found them. This has showed me the necessity of paying a closer attention to symptoms, especially where there is an easily demonstrable pelvic disease, so as not to be misled into the belief that the disease for which I am about to operate is, of necessity, the only thing that ails my patient.

My experience shows that gallstones are by no means as frequent in this country as represented by the autopsies made in Europe, where they are said to be as common as 1 in 10.

I believe in doing a cholecystectomy when the gall-bladder is grossly diseased and it is not too difficult to always do an ideal operation. I recall two recent cases in which this could not be done, and I could not remove the gall-bladder because of the density of the adhesions. One patient had a stone in the common duct, with empyema of the gall-bladder, the stone being so low down in the common duct that I could not get it out through an incision in the fundus of the gall-bladder. I then, with difficulty, freed the gall-bladder at its left upper surface, and then continued the incision down from the top of the gall-bladder into the duct and so extracted the stone. I could not extirpate the great, thick gall-bladder with safety without running the risk of impairing the common duct. I therefore concluded the operation by cutting out the lining membrane of the gall-bladder and burning it with a Paquelin cautery, and then stitching it into the abdominal wound. The patient has made a good recovery without a fistula.

In another patient with empyema of the gall-bladder, full of gallstones and with a large stone in the common duct, I took out, first, the common duct stone and then opened and cleaned out the gall-bladder. Finding I could not, without too much destruction of the adjacent tissue, take out the gall-

bladder, with the stones, I enucleated the stones in this way: I first opened the gall-bladder below, separated the mucosa and some of the submucosa from its bed, and thus brought out the stones with the empyema unruptured, at first effecting an intracystic enucleation of the stones without doing a complete cystectomy.

Dr. Charles H. Mayo, of Rochester, Minn.—Speaking of symptoms without gallstones, in our clinic we occasionally operate, after having made as thorough an examination as possible and thinking we had a case of gallstones to deal with, without finding stones. It is rare that we diagnose an intra-abdominal condition to the extent of believing that there is disease of the bile ducts or gallstones and not find some condition in the abdomen which would have made it necessary to operate. I do not think a patient cares whether he has gallstones, duodenal ulcer, or ulcer of the pylorus, if he can be relieved. He has no special preference, although it is nice to have the gallstones in a bottle and show them to his friends, because it proves he had that trouble.

Some years ago we operated and finding a blue gall-bladder, which is ordinarily considered a healthy gall-bladder, but covered with adhesions, as Dr. Morris has said, we drained such gall-bladders and tried to relieve the condition, the adhesions. One of these patients was operated on a year or so afterward, one two years afterward for the same condition, and at that time we found duodenal ulcer had been the cause of adhesion, which was overlooked at the first operation. In the case of a healthy gall-bladder, covered by adhesions, always examine the duodenum and stomach for ulcers. Such ulcers are oftentimes overlooked. Ulcers may mean stricture. Dilatation of the pylorus may make it appear as part of the stomach, the source of the stricture being lower. These cases have for years been called ulcer of the stomach. They are mistakes in diagnosis as far as location is concerned. It is not easy sometimes to make a differential diagnosis between duodenal ulcer and disease of the gall-bladder because the same set of nerves supplies both of these regions. It is difficult to tell whether we have a duodenal ulcer or ulcer of the stomach; but rarely do we make a mistake in differentiating stomach ulcer and gall-duct disease. So with Dietl's crisis. We can produce such crisis by putting a patient on the table, and with a ureteral catheter dilate the kidneys. If a patient

has a movable kidney which is producing obstruction and you dilate the pelvis of the kidney and produce the exact symptoms mentioned, it is all right to operate on that kidney. If you cannot, it is best to examine the gall-ducts and the appendix before you quit. If you have made a diagnosis of gallstone disease, you find you have been mistaken in your diagnosis, it is best to explore the abdomen thoroughly, and it means a good deal of work to make a thorough examination of everything, including the kidney. A second operation may be required. To secure good results these operations are not as simple as Dr. Price would have you to believe, and which, he says, can be done before breakfast.

Dr. Richardson (closing the discussion on his part).—I have been very much pleased with this discussion, though it has included practically all branches of gall-bladder surgery when I intended to bring up for discussion chiefly the diagnosis.

Not many years ago it was the custom for many of our most eminent surgeons to introduce their etherized patients upon the operating table by saying, "Gentlemen, this is said to be a case of so-and-so. I have never seen the patient before," and he would proceed to operate. I refer to the early antiseptic era in the very beginning of the development of abdominal surgery. Even today, one occasionally hears such a remark. A speech of this kind indicates that the surgeon has never given his patient the careful attention which every case, even the apparently most trivial, deserves—not to say demands. It means that sooner or later disaster will occur from some serious but undiscovered contraindication. It means, furthermore, a reproach to surgery, when the surgeon admits that he has had his diagnosis made for him and that he is but a tool in the hands of the physician. In the past; we, as surgeons, have far too often had diagnoses made for us, especially in abdominal diseases. The first half of my paper was therefore devoted to diagnosis, and was illustrated especially by the mistakes I have been able to find in the last year or two. We have no business to operate unless we ourselves believe that we have good reason for operating.

The burden of my remarks is that it is our duty to study cases with the greatest care, but it was not my intention to bring up technical questions.

With reference to drainage of the hepatic duct it is abso-

lutely indefensible as a routine procedure, except after choledochotomy and occasionally after cholecystectomy. Whenever it is essential to drain the hepatic radicles, and when for any reason it is impossible to do so through the gall-bladder or the cystic duct, a tube must be placed in the hepatic duct. Drainage of the dilated hepatic duct is easy, for the opening is large and the walls are thick and the duct is easy to find; drainage of the normal duct is exceedingly difficult, for the opening is small and the walls are thin and the duct hard to find. Furthermore, perfectly satisfactory drainage is possible, in the great majority of cases, through the gall-bladder or the cystic duct.

With regard to cholecystectomy, I believe pathology shows that the gall-bladder, diseased for years and years, may produce no serious symptoms. This is shown by the contracted and useless gall-bladders which we find in a large proportion of old cases. In these cases the gall-bladder has not for years been used as a receptacle of bile, and the chief indication for its removal is a chronic grinding pain such as I have described.

Dr. Price said that in some cases the symptoms are obscure, and we do not know what they mean. When symptoms caused by gallstones are obscure we shall usually find extensive and serious anatomical changes. The chief indication is acute or chronic cholecystitis which justifies the removal of the gall-bladder. The results, immediate as well as remote, are good. It is very exceptional for a biliary fistula to follow extirpation of the gall-bladder for any cause. When, in acute infections of the gall-bladder, simple drainage is used to tide the patient over an emergency so serious that prolonged exploration is unjustifiable, a stone in the cystic duct will be overlooked. A fistula, intermittently closing and opening, discharging clear mucus, will indicate that the cystic duct is closed, probably by a stone. When the gall-bladder is perceptible through the abdominal wall, gallstones will almost always be found in the cystic duct. In the great majority of cases of stone or stones in the cystic duct, the only thing to do, in my judgment, is to extirpate the gall-bladder, duct and all.

Dr. Stone (closing).—I wish to call attention to one point in connection with Dr. Richardson's paper in regard to the symptoms. In recent papers we find that reference is made

to wandering gallstones. Of course, they are all neglected cases where the stones have ulcerated their way through into the peritoneal cavity; into the intestine or elsewhere. One of the most important cases I have seen within the last year was one of that kind. It was reported to our local society. In this case one gallstone had worked its way through the duodenum and got into the stomach, leaving an open tract between the gall-bladder and stomach. The other gallstone had worked its way into the transverse colon followed by hemorrhage enough to fill the entire intestine, and yet the man had never had symptoms of gallstones, nor had he ever had jaundice or any biliary symptoms.

With reference to the remarks of the various gentlemen who have spoken, Dr. Lloyd did not say whether he drained the common duct or the cystic duct, but I suppose, of course, like most surgeons, he practises drainage in infected cases. I see a good many cases of infected hepatic ducts. The gall-bladder may not be markedly diseased. We see nothing the matter with the bile; it appears to be healthy. However, there is something wrong, because the temperature promptly goes down after drainage of the gall-bladder has been instituted.

The more I see of these cases of cholecystitis, the more I am induced to study the cause of the trouble, and I usually find more deep-seated trouble than the mere presence of gallstones. Eventually, I think we will, by giving these cases greater attention and closer inspection, learn how to search for gallstones in the hepatic or pancreatic ducts and we will be able to permanently cure a class of patients which at the present time are not relieved by either cholecystotomy or cholecystectomy.

# THE MANAGEMENT OF LAPAROTOMY PATIENTS AND THEIR MODIFIED AFTER-TREATMENT.

By H. J. Boldt, M.D.,
*New York.*

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Our object when instituting any form of treatment for a patient is to bring about restoration to health and working ability as soon as possible. When we do an operation we employ such technique as will, in our opinion, assure the greatest degree of safety to a patient, and then conduct the subsequent treatment in such manner as will ward off the complications and undesirable consequences incidental to operations.

It is my chief purpose in this contribution to consider the treatment of patients upon whom abdominal sections have been done. While there has been progress in everything connected with medicine and surgery, yet the management of such patients has practically remained unchanged during the past few years. Elaborate articles have appeared in text-books and journals as to how patients should be cared for after abdominal sections; as to how long they should be restrained from moving about in bed; as to how long they should remain in bed; as to how they should be dieted, etc.

The customary treatment, in all its details, is dealt with in a most efficient manner by Howard Kelly in the second edition of his *Operative Gynecology*.

Before considering the special subject of my discourse it may be opportune to state in a general way the man-

agement which I make use of for patients upon whom I purpose to do a laparotomy. The preparation of the patient is very simple. They get no special diet unless there is a special indication for it. On the evening before the operation a warm bath is given if convenient. The abdomen is then shaved and the vagina is thoroughly irrigated with a mild antiseptic solution. If the bowels have been regular, nothing is done toward further emptying them except to have a nurse give a copious enema eight or ten hours prior to the operation. Over night an antiseptic dressing is fastened upon the abdomen. Two hours and a half before the operation a subcutaneous injection is made of $\frac{1}{180}$ of a grain of scopolamine hydrochloride with $\frac{1}{12}$ of a grain of morphine sulphate. This medication is repeated an hour later, and again half an hour before the time set for the operation. The effect of it is that the patients usually require less ether. On two occasions I have been able to do major operations without any other anesthetic, and in another instance not more than about 15 c.c. of ether was required toward the termination of the operation. I have not, however, been able to observe anesthesia so generally as has been published in the reports of others, to permit of operations without resorting to general anesthesia with ether or chloroform. I deem it my duty in this connection to state that I have been unfortunate enough to have two patients, in whom the preliminary scopolamine-morphine narcosis was induced, die on the table during the operation, practically with only a few moments' warning, manifested by a sudden change in the pulse, which became very slow and almost imperceptible. In the second case respiration also ceased almost simultaneously. On one of these patients an autopsy was made, but nothing was found to account for the death, even on microscopic examination of the heart, large blood-vessels, kidneys, and liver. In the other case an autopsy could not be procured. Whether the scopolamine-morphine

narcosis contributed toward the fatal termination, I am unable to decide. I think, however, that in the case of the patient upon whom an autopsy was made with an entirely negative result the absence of a discoverable pathological lesion would point to that inference.

A short time before the operation I prefer to have a cathartic administered so that its effective action may manifest itself from twelve to fourteen hours after the operation; however, if the patient's bowels have been regular previously, the cathartic is omitted. *It is insisted upon that a patient shall not needlessly be kept under anesthesia. The prevailing custom of keeping a patient under full anesthesia fifteen minutes or more before an operation is begun, cannot be too severely criticised.* A patient need not be under complete anesthesia before the final abdominal cleansing is begun. The operator, assistants, and instruments should be in readiness, so that when the anesthesia is complete the operation may be begun.

The incision is made on one side of the median line, the corresponding rectus muscle being separated by dull dissection with the handle of a scalpel, and the forefingers are then used to complete the separation for the entire length of the incision. The incision should be made sufficiently long to permit of easily accessible work on the seat of the pathological condition for which the laparotomy is done, because it stands to reason that when one can see, as for instance, in the separation of adhesions, the manipulation is safer than when if one works entirely in the dark. The fact that an incision is an inch or two longer is of no significance so far as the healing of the abdominal wound is concerned, or so far as concerns the probability of the subsequent occurrence of ventral hernia, if the incision is properly closed; in fact we are likely to secure primary union of such a wound more readily than of one that has been abused by traumatism during manipulation, as is the case in too short a wound.

Before the peritoneum is closed, the omentum should be carefully spread over the intestines. Only in exceptional instances is the abdomen washed out, flushed, with saline solution. In purulent cases, the general peritoneal cavity is walled off, that is protected from contamination, with towels or a heavy gauzeroller-bandage about four inches wide and folded about eight layers thick. Pus is mopped out with pads. Abdominal drainage is made use of only in instances of diffuse oozing from torn adhesion surfaces, and then preference is given to vaginal drainage, so that the abdominal wound may be completely closed.

The abdominal wound is closed in tiers—first, the peritoneum, then the posterior sheath of the rectus in instances of long wounds, next the separated rectus muscle with interrupted sutures, next the fascia by overlapping it, so as to have a broader surface of coaptation. The skin is closed with a subcutaneous suture. Chromicized catgut is used for the fascia; for all other suturing plain catgut is used.

Apropos of catgut, which is one of the most important factors in an operation when one makes use of it as suture material and ligatures, it was my misfortune a short time ago to lose two patients with tetanus. The first instance was after an operation for a very large ovarian tumor, which was adherent throughout, in a woman fifty-five years old. The woman had been up and walking about for several days and was to be discharged two days later, when, on the twelfth day, she showed the first tetanus symptoms. She died six days later. The second case was one of abdominal hysterectomy for a large myofibroma, in which the first symptoms of tetanus showed themselves on the ninth day, and to which the patient succumbed three days later. The cultures taken from the reopened wound revealed tetanus bacilli. The suspicion that the catgut was the carrier of the infection was but natural. I therefore had a number of tubes examined by Dr. Muenchehofe, of the Museum of Natural History, through the Kny-Scheerer Co., of which the

results were negative, as will be seen by the accompanying report.

THE KNY-SCHEERER CO.:

Gentlemen: The different kinds of catgut you sent me I have examined and find the following: In twelve tubes of catgut marked *Customers' Samples*, after having made ærobic and anærobic cultures in the different media, gelatin, glycerin, and glucose agar, also culture plates, I find that after ten days in the incubator, of half of the culture and plates, there is no growth visible. It is the same with the ordinary room temperature.

From three tubes each, marked *Stock*, the same cultures and plates were made, after ten days at incubator of half of them, the other half at room temperature, the result being the same as in the foregoing.

From another lot of open tubes asta chromicized catgut and kangaroo tendons, five were inoculated with anthrax bacilli and spores; five others with tetanus bacilli and spores; five more with hay bacillus. All were then sealed and subjected on three successive days to your method of sterilization, after which time the usual bacteriological tests were made of ærobic and anærobic cultures and plates in the different culture media and were kept in incubator, partly at room temperature; after ten days no growth was visible in any of them. The results obtained show that all the different tubes marked *Customers' Sample* were sterile; the same with samples from *Stock* asta chromicized and kangaroo tendons.

It also demonstrates that the asta chromicized and kangaroo tendons which were infected with the very resistant spores of anthrax and tetanus bacilli were destroyed. The facts brought out with these experiments show that your method of sterilization is doubtless sufficient and an absolute proof against infection with any kind of bacteria.

Yours respectfully (Signed).

C. W. F. MUENCHEHOFE, M.D.

To make doubly sure as to the effect of fractional sterilization on positively infected catgut, I requested Dr. Arthur Mandel, of the Cornell University Medical College, to inoculate some catgut, and he soaked it, after untwisting it, with the view of getting it thoroughly inbued with the bacilli and spores of anthrax and of tetanus and hay bacilli, for twelve hours in bouillon which had been thoroughly infected with the microörganisms named. The infected catgut was replaced in tubes and sealed. It was then sterilized in the Kny-Scheerer laboratory and returned to Dr.

Mandel for examination. After every method of examination it was found absolutely sterile. These tests had the object of proving or disproving conclusively whether catgut which contained very resistant microörganisms, and which was subsequently properly sterilized, was absolutely reliable so far as the possibility of causing infection was concerned.

At the meeting of this society held in Birmingham, two years ago, I said a few words regarding the subsequent treatment of abdominal section patients, but the treatment was then still in the experimental stage. The first premeditated important departure from the long rest in bed after an abdominal section was made by Dr. Emil Ries, of Chicago. Believing Ries to be conscientious and a good observer, I had no hesitation in putting the departure to the test. I added, however, one modification which I considered of the utmost importance for the patient's abdominal wound; in fact I believe it to be indispensable, namely, a proper abdominal binder, which Ries does not consider necessary.

I may add that in a letter dated November 17, 1906, Dr. Robert T. Morris states that he for a time required his patients to arise thus early from bed after abdominal sections fifteen years ago, but later discarded the practice because of the severe criticism to which he was subjected by some of his colleagues in the hospital with which we are connected. I regret not to have heard of it, until his communication reached me. Morris did not publish his work in this direction. Ries' work, however, was published in the *Journal of the American Medical Association* in August, 1899.

When I first deliberately risked permitting patients to get out of bed in less than three days after abdominal operations, I recalled several instances of extensive operations in the abdominal cavity, in which the patients, through carelessness on part of the nurse, got out of bed within twenty-four hours, even in the days when we used hard rubber and glass drainage tubes, and these patients had such a tube in the abdomen, and yet they were none the worse off in conse-

quence of their rashness. I further recall one patient who persisted in getting out of bed and walking about after the third day whenever the nurse was absent. Also, that for more than fifteen years my patients had been permitted to move about in bed as much as they pleased after abdominal operations, unless there was a strong contraindication, and yet I failed to observe any harm resulting from such exertions. Further, from the fact that others have had patients who also, without permission, left their beds soon after he operation without any ill effects, and, lastly, that since 1890 it has been my custom to permit my patients, upon whom I had done a vaginal hysterectomy, to walk about any time after the first day if their condition was favorable, and have not seen an instance of unfavorable result. One may naturally ask himself the question: Is there really danger in a patient's getting out of bed soon after an abdominal operation if her general condition is fair and proper precautions are taken? I am aware that advocating the treatment as routine practice will find much opposition, so much so that one of my former clinical assistants read a paper before the New York Obstetrical Society against my teaching, but for unately my advocacy is based upon experience, while his paper was based only upon theory.

Since my first remarks uttered in public regarding this change of after-treatment, I have had ample opportunity for studying the effects of the method which I so warmly advocate, and which I now consider beyond the experimental stage, indeed experience has taught me that the method of treatment which I propose to speak of is good, better than the customary method, but, while it is simple, yet much more discrimination in selecting patients upon whom to carry it out is required than if we treat them in the way usually laid down in text-books.

After the completion of a laparatomy, a simple dressing is placed over the site of the abdominal incision, consisting of a strip of sterile gauze two and one-half to three inches

in width and six to eight layers in thickness; this is fastened with two short, narrow strips of adhesive plaster, simply to hold it in place while the binder to immobilize the abdominal parietes is adjusted. *The bandages used by me to immobilize the abdominal parietes is a Scultetus bandage, for which zinc oxide plaster is used. The full width of the plaster,* 12 *inches wide, is used, and according to the size of the patient, from* 28 *to* 40 *inches or more in length. At the bottom, a small half circle is cut out in the middle, so that during defecation the bandage is not soiled.* The patient is now placed upon the bandage so that the lower border comes about on a level with the head of the femur. Now the fabric covering on the plaster is taken off, and when that part of the plaster is reached upon which the patient is resting, the patient is rolled over a little on one side to facilitate the complete removal of the covering, the end of which is now taken hold of by the assistant on the other side with both hands, and the plaster is entirely freed of the covering. I have found this better than to remove the covering before placing the patient upon the plaster, for it makes wrinkling of the plaster less likely. Next the plaster is cut on each end into four equal widths and torn as far as the body of the patient; thus the body of the plaster is upon the patient's back; in short, we now have a many tailed bandage made of oxide of zinc plaster. The zinc plaster is preferable to rubber plaster because of its less irritating qualities. The tails are now *snugly* adjusted, beginning with the *lower* one and bringing it to the opposite side, then the opposite side is overlapped all the way down to the other side of the patient, thus making a double structure in front and at the sides. This is continued until the four tails on each side are fastened. If the upper part of the bandage should reach up to the epigastrium in short patients, the topmost tails are not drawn so tight. In thin subjects the anterior superior spine of the ilium is slightly padded with gauze.

Such a bandage immobilizes the abdomen to such a degree

that it is impossible for the abdominal wound to give way as the result of intra-abdominal strain. I consider the bandage a most important adjunct part; the main precaution in the after-treatment. In the beginning patients usually complain of the tightness of the bandage, but this generally soon wears off. If it is really unbearable for the patient, the upper strip may be cut a little on each side. Of their own accord patients seldom care to get out of bed until the third or fourth day, yet it has been my custom to help them in all simple cases out of bed within twenty-four hours and have them sit in a comfortable chair, and to coax them out subsequently as much as possible. Occasionally, I have done abdominal hysterectomies for fibroids, panhysterectomies for purulent inflammatory conditions, ovarian tumor operations, etc., in the morning, and have had my patients out of bed in a rocking chair late in the afternoon of the same day, this depending entirely upon the general physical condition of the patient before the operation and upon the effects of the anesthesia. My patients receive *no special diet unless there are particular contraindications for regular diet; they get food such as they had before the operation as soon as the stomach is in condition to retain food after the anesthesia.* By the end of the fourth or fifth day the patients, after *uncomplicated* abdominal sections, usually walk about as though they had had no operation done upon them. If there is no indication to inspect the wound, the binder is not taken off for three weeks, sometimes four weeks. If the bandage is uncomfortable from wrinkling, or loose, it is removed after two weeks or whenever it seems necessary, and replaced by another after the skin has been left free of a bandage for a day, during which time the patient is bathed with alcohol and the skin powdered with talcum two or three times. The patients usually go home with the bandage, which is permanently removed at the expiration of about four weeks. If there is reason to suspect suppuration of the abdominal wound, the wound is inspected by cutting the bandage in

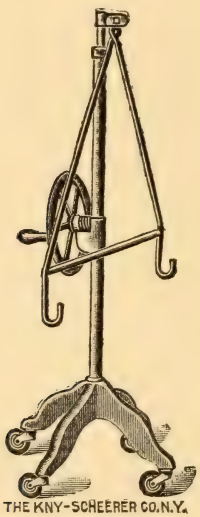
the median line over the gauze layers; this can be done without any marked discomfort to the patient.

Should suppuration be found to be present, then the wound is taken care of in the ordinary way and the patient kept in bed until the suppuration has ceased. The cessation of suppuration is hastened by swabbing the suppurating surface with pure carbolic acid and immediately washing it off with alcohol. If no suppuration is found the bandage is again fastened by applying other strips of plaster over it.

Exceptions to the inducement of early rising, that is, within twenty-four to forty-eight hours, are made when physical condition has been much weakened by illness prior to the operation, or when the pulse rate is much above the normal, or when the operation has been one of unusual magnitude; then, but only then, no attempt is made to get them out of bed before the end of the fourth day, or even then if there is any indication for unusual care. Lately, however, I have even deviated from this and taken the subjects, in the most complicated cases, out of bed after from twenty-four to thirty-six hours, unless they were really too feeble, or unless there was un unusual complication, such, for instance, as happened very recently, the necessity for a ureteral implantation in connection with a difficult hysterectomy for a retroperitoneal myofibroma, which in my opinion made it desirable to keep a permanent catheter in the bladder for four days; but at the end of the fifth day she, too, was sitting out of bed.

In those cases in which the operations were complicated, or in which peritonitis was present before the operation, or in which it was obvious that pathological intraperitoneal secretion would form, so that it was desirable to localize such secretion in the pelvis, or in instances of the first sign of a beginning peritonitis, so that in my opinion it was too hazardous to have such patients out of bed, I have used a bed lifter which I have designed and had constructed to enable me to obtain the trunk elevation

recommended by the late Dr. George R. Fowler. It is constructed so that the head of the bed is let into two strong hooks attached to the frame, which is attached to a rod that can be elevated or lowered to any desirable height without physical exertion by the patient, by means of a cog wheel and a large hand wheel, as shown in the illustration. It is on the same principle that I have had my office treatment or examining table built. I have found this a valuable appliance in all such cases; it also facilitates vaginal drainage.




Bed lifter.

Patients who are very anemic and in a poor physical condition will be relieved from the immediate effects of the operation if a tight bandage is placed around the upper part of the thighs before the operation is begun, to act as a tourniquet, thus temporarily keeping the blood in the lower extremities out of the general circulation. As soon as the operation is completed the bandage is loosened and the extremities are bandaged from below upward for a few hours, so as to get more blood into the trunk.

With my present views, the principal objection to per-

mitting patients after abdominal section to arise from bed early and go about, is the danger of a subsequent ventral hernia, or perhaps even worse, of having the wound burst open from the effects of intra-abdominal pressure—certainly not because of the surgical work done in the pelvis or the danger of thrombosis, because for more than fifteen years I have permitted my vaginal hysterectomy patients to get out of bed and walk about within twenty-four to forty-eight hours if the vaginal vault had been closed at the time of the operation, or if only a small strip of gauze had been inserted in the centre of the wound for the purpose of drainage during the first twenty-four hours, and all these patients were in a better physical condition at the end of two weeks than those who had been confined to their beds for ten days or longer. So far as the danger to the abdominal wound is concerned, it is safely and positively overcome by the proper closing of the abdominal parietes and the adjustment of the bandage which has been advised.

I have found it beneficial to have the stomach thoroughly washed out as soon as the operation is finished. The stomach lavage, while the patient is still on the table, is readily managed and saves a patient much nausea and retching.

In many instances one dose of morphine is administered soon after the operation, because it is seldom that a patient does not complain of sufficient pain to make such treatment not only humane but beneficial to the patient, acting far better on the heart under some circumstances than strychnine.

I believe that strychnine is used far too extensively and indiscriminately, both during and after operations, whereas, intravenous saline infusions are often too long delayed, and the administration of a dose of morphine after an operation is too much dreaded by many.

If the bowels do not move spontaneously within twenty-four hours after the operation as the result of the cathartic not infrequently given before it, no attempt is made to bring about an action, because usually this occurs spontaneously

about the fourth or fifth day, and should it not, then, if the patient feels uncomfortable, a saline laxative is given.

I have been frequently asked, What is the object in getting patients out of bed so soon? I ask in return, What is the object in requiring patients to remain quietly in bed so long? The reasons stated in text-books for the latter procedure as a routine method do not, from my observation, hold good in practice when compared with the results achieved by the course which I have the privilege to advocate at this meeting. For most abdominal sections those reasons for the necessity of enforced quietude in bed must be regarded as theoretical.

Ries wrote me on November 1, 1906, that he had used the heretical after-treatment of permitting patients upon whom he had done an abdominal operation in more than 500 cases without having observed any untoward result that he could attribute to it. I have had 384 cases and not a single instance of untoward result which could in any way be attributed to the early arising from bed. I know of others who upon my suggestion, after seeing some of my patients, also permitted their patients to get out of bed, although not so early, usually on the third or fourth day, so as to bring the total number up to more than 1000 cases; and yet no unfavorable result has been reported to me. This seems to me a sufficiently large number of cases to demonstrate whether the treatment has merit or whether it is fraught with danger. The cases chosen by me were not simple ones alone, but they were taken irrespective of the complications which were met with. For instance:

C. McG. Double salpingectomy, for double tubal pregnancy; left tube ruptured, the right was unruptured. Patient out of bed most of the time on and after the fourth day.

J. W. Double ovariotomy for cancerous ovarian tumors; exsection of the right broad ligament; panhysterectomy because of malignant involvement of the uterus; exsection of the lower part of the sigmoid flexure and upper part of

the rectum, because of carcinomatous involvement; extirpation of a large part of the omentum; extirpation of many retroperitoneal glands. Patient out of bed most of the time beginning with the sixth day.

F. K. Tuberculous peritonitis; catarrhal appendicitis. Out of bed on the fifth day.

P. R. Complicated panhysterosalpingo-oöphorectomy. Out on the second day.

C. K. Abdominal hysterectomy for malignant chorioma. Out of bed within twenty-four hours. Discharged on the eighth day.

M. G. Abdominal myomectomy; acute appendicitis. Out of bed six hours after operation.

M. S. Complicated abdominal hysterosalpingo-oöphorectomy; intense pelveoperitonitis present. Out of bed on the third day.

I. McC. Similar operation for a similar condition. Out of bed on the third day.

F. K. Very adherent retroflexion; annexa likewise adherent. Out on the second day.

B. B. Complicated panhysterectomy. Out within twenty-four hours.

E. O. Exsection of the left broad ligament with annexa; ventral suspension and appendectomy. Out on the second day.

J. B. Hysterosalpingo-oöphorectomy, very complicated with extensive injury of the bladder. Out on the fifth day. Discharged on the twelfth day.

M. S. Panhysterosalpingo-oöphorectomy; diffuse septic peritonitis. Abortion case. Out on the third day.

F. L. Myomectomy. Out within twenty-four hours.

E. L. Complicated bilateral salpingectomy; ventral suspension; appendectomy. Out after the second day.

M. D. Panhysterectomy, with resection of the upper third of the vagina; both broad ligaments and both ureters exposed in their pelvic part, the right ureter cut and

anastomosed. Retroperitoneal glands removed. Appendectomy. Carcinoma case, far advanced. Out on the fourth day.

J. G. Double ovariotomy for cancerous tumors; panhysterectomy. Out on the following day and the day thereafter; when on the next day the patient was kept in bed because she began to show evidence of peritonitis, of which she died on the fifth day.

E. D. Hysterectomy for large fibroid with adherent suppurating tubes. Out within twenty-four hours.

S. E. Hysterosalpingo-oöphorectomy for adenocarcinoma of the ovaries. At time of the operation it was thought that tuberculous peritonitis was present, but it proved a carcinomatous peritonitis. Out of bed within twenty-four hours. From a report later received the patient lived about six months before succumbing to her ailment.

S. L. Adherent ovarian tumor. Out of bed on the same day.

G. P. Tubo-ovarian abscess with diffuse adhesions; ventral fixation. Out on the next day. Was later returned to bed because of wound infection.

E. G. Adherent dermoid tumor of ovary. Salpingectomy on the opposite side with plastic work on the ovary. Out the next day.

J. E. Complicated extrauterine pregnancy. Out on the fourth day.

S. F. Complicated panhysterectomy. Out on the next day.

R. G. Ruptured tubal gestation. Out in twenty-six hours.

R. K. Complicated panhysterectomy for intraligamentous myofibroma. Out on the third day.

C. K. Hysterectomy for large myofibroma with pregnancy. Out the next day. Was subsequently returned to bed for two days because of bleeding.

M. M. Hysterectomy for large myofibroma with double pyosalpinx. Diffuse adhesions. Out on the fourth day.

A. K. Suppurative appendicitis with beginning peritonitis. Long median incision to umbilicus. Up the next day. Returned to bed on the sixth day because of wound infection.

G. S. Complicated salpingectomy for pyosalpinx; appendectomy. Curetting. Up the next day.

L. N. Similar operation. Up the second day, but was later returned to bed because of wound infection.

V. S. Very complicated hysterectomy for myofibroma with double pyosalpinx; appendectomy. Up the next day A *very* stout woman.

E. S. Complicated tubal pregnancy. Up on the second day.

J. R. Complicated panhysterectomy. Up on the second day.

S. K. Complicated bilateral salpingectomy; appendectomy; ventral fixation. Up on the fifth day.

M. L. Ovarian tumor with diffuse adhesions; appendectomy; ventral suspension. Up on the next day.

E. N. Curetting; trachelorrhaphy; anterior colporrhaphy; bilateral posterior colporrhaphy; right ovariotomy for multilocular ovarian cyst; resection of small multilocular ovarian cyst (three inches in diameter) on the left side, retaining some seemingly normal ovarian stroma; appendectomy; ventral suspension. Up the next day but returned to bed again for a few days after the fourth day, because of mild abdominal wound suppuration

F. K. Large ovarian tumor (left) with twisted pedicle, five times from left to right, with torsion of 25 degrees at cervix from left to right; much oozing from extensive peritoneal adhesions. Out the next day.

A. F. Double ovariotomy. One of the tumors quite adherent. Retained some ovarian stroma on the other side. Appendectomy. Up the next day.

J. W. Right tubal gestation; appendix inflamed and adherent to the tube, also removed. Up on the second

day, but was later returned to bed for a few days because of wound infection.

J. S. Very large ovarian tumor, adherent throughout. Up the next day. Patient manifested tetanus symptoms on the twelfth day and died six days thereafter.

R. M. Complicated hysterosalpingo-oöphorectomy and appendectomy. Up the next day, but was later returned to bed because of wound infection.

H. L. Panhysterosalpingo-oöphorectomy; septic case from criminal abortion; ovarian abscesses and peritonitis. Up on the second day, but was then returned to bed again because of the pulse rate and temperature, 102° F. Definitely out of bed from the sixth day.

K. D. Hysterosalpingo-oöphorectomy and appendectomy. Annexa very adherent. Up on the second day.

R. K. Ruptured right ovarian tumor causing diffuse peritonitis; emergency operation during night. Out of bed on the third day, but later returned because of wound infection. It was macroscopically a papillomatous neoplasm, which later proved carcinomatous.

S. K. Large myofibroma with double pyosalpinx; intimate adhesions. Very stout woman. Up the next day.

H. K. Complicated hysterectomy for myofibroma and appendectomy. An unusually stout woman. Up the next day. Nearly four weeks later the first indication of deep-wound infection was evident, for which she was then kept in bed at her home for about two weeks.

R. O. Panhysterectomy for large intraligamentous myofibroma; appendectomy. Up on the second day.

A. A. Panhysterectomy for myofibroma. Complicated case. Up the third day.

R. W. Hysterosalpingo-oöphorectomy; double ovarian abscess; on one side pyosalpinx and on the other side hydrosalpinx; many adhesions. Up on the fourth day.

In the foregoing short list of 50 cases, as may be seen, I have not included any of the simple abdominal sections

for ovarian tumors without adhesions, noncomplicated hysterectomies for fibroids, easy operations for ectopic pregnancy, etc. Nor do I deem it necessary to cite all the instances of technically difficult cases treated by the method advocated, because it would be too tedious. My object is to cite only a sufficient number of cases, so that those interested in this work may know the kind of cases that have been so treated. The method is applicable in about 85 to 90 per cent. of patients subjected to abdominal section by gynecologists. On the first day or two that patients are taken out of bed, they usually walk only a few steps, from the bed to the chair, and then remain in their chair. They are allowed to remain up as long as they wish; on the first occasion the time is from three-quarters of an hour to two hours, and in the afternoon this is again repeated. Every day, however, the time of being up is increased, likewise the amount of exercise. Of course, as might be expected, patients sometimes protest, but a little coaxing usually induces them to acquiesce. It is certainly remarkable to see the physical condition in which such patients are after two weeks have elapsed, compared with that of those who were kept in bed on the customary treatment. Those whom, for various reasons, I think best to keep in bed, I endeavor to have do some calisthenic work with their upper and lower extremities while still on their back, unless, in my opinion, they are too prostrate. Five minutes every hour or two helps to keep the lethargy out of their muscles and causes better circulation.

It is of decidedly more benefit to a patient under ordinary circumstances to allow her to spend the first week partly in bed, and to induce her to take moderate exercise, than to let her remain in bed two or three weeks and then begin to get her up. Most patients, upon whom operations are done for primary surgical diseases, do not have their health improved by bed rest, but on the contrary their muscular system becomes more or less atrophied and they lose strength,

because all the physiological functions become more or less impaired by the enforced quietude. The only class of patients who would be benefited by bed rest are extreme neurasthenics, and *they only if they are properly fed and in addition get methodical general massage daily.* Take, for instance, malignant disease which has undermined the patient's general physical condition, and where on operation the disease is shown to be advanced too far for us to hope for permanent cure, or even to attempt to do a radical operation for its cure; such patients, if restrained, are very likely to become permanently bedridden, at least such has been my experience in a number of such cases. On the other hand, induce them to get out of bed soon, if possible within the first twenty-four hours, and their physique is not undermined more than by the ordinary natural course of the disease.

During the past two weeks I have had two additional instances to prove this. One I shall mention.

J. K., forty-nine years old, cachectic appearance; the abdomen greatly distended, which gave rise to the erroneous diagnosis, on the part of several physicians before she was referred to me, that she had a very large ovarian cyst. The pelvis was filled by a hard mass, which on the surface, palpable per vaginam, had a number of small irregular nodules. The uterus and annexa could not be palpated because of the enormous abdominal distention. The diagnosis, however, was made of probably malignant disease, yet there was a slight possibility that I might be mistaken and that the condition might be tuberculous peritonitis, consequently an exploratory section was made by me on December 3. Unfortunately the first impression proved correct. The omentum was carcinomatous, and numerous carcinomatous nodules were in the mesentery. The mass in the pelvis was composed of coils of intestine with malignant deposits between them. A cigarette drain was placed in the lower angle of the wound to temporarily get the woman rid of the

ascitic fluid, and the abdomen was closed; the Scultetus bandage applied above the drain. On December 5, the patient was up and about. On December 6, she walked about *much* better than she did before the exploratory section, and in every way felt better than she did previously, besides, the haggard expression on her face had much improved.

The operations that I have done on the intestinal tract are few compared with those which a busy general surgeon does, yet they have been sufficient in number to teach me that even such operations are no contraindication to the treatment advocated.

I am frequently confronted by doubters and antagonists of the method with the rule that we usually insist upon in obstetric cases. In these we have altogether a different condition to contend with. There is a large, heavy uterus with stretched ligaments and a relaxed pelvic floor; these must have time to regain their former condition or else displacements are likely to follow. All this is different in strictly surgical cases. In instances of large tumors the oxide of zinc Scultetus bandage gives adequate support to the abdomen and assures absolute protection of the abdominal wound, provided, of course, that there is no wound infection. It has been suggested that, perhaps, just this treatment is likely to cause suppuration of the wound. For a time this thought also came to my mind, that perhaps, from the mobility of the patients, occasionally the irritation of chromicized catgut knots might be a causative factor of suppuration; yet, on the other hand, not only I but also my colleagues, had under the employment of the customary treatment, at some time seen almost a small epidemic of wound infections in hospital practice, whereas, in strictly private practice I have not seen a single instance of it during the past five or six years. Again, in those cases in which I have used catgut sterilized by the fractional method of sterilization, it has rarely occurred, never in a clean case. I do not attribute the fortunate recovery in my very serious

cases to "luck" despite my having used such heretical treatment. On the contrary, I believe that it was because I used the newer, or modified treatment, that my patients got well; hence my opinion that there is a lower mortality with this treatment.

I have also thought of the medicolegal side of this question, and fail to see why the man who makes use of it should be condemned by his confrères if he should have a patient die of embolism a few days after an operation, or because the patient was attacked with thrombosis. It would have to be shown that patients who were kept abed did not have phlebitis, thrombosis, or embolism. I maintain that these accidents are not so likely to occur if the treatment, a rational activity of the patient after operation, is insisted upon. It is proved by experience.

Dr. Marshall and Dr. Quick, in an article that appeared in the *Medical Record* of November 24, 1906, are also advocating more and earlier mobility for their *simple* laparotomy cases. The greater advantage, however, I am sure will be seen if the treatment is extended to the really complicated cases, just that class of cases in which I also formerly insisted upon absolute rest for two or three weeks or more. Particularly in complicated panhysterectomies with suppurating tubo-ovarian inflammation, with or without vaginal drainage, I have found the method advocated of very superior advantage. The vaginal gauze drain does not prohibit a patient from sitting up and taking light exercise. Besides, in from twenty-four to forty-eight hours the packing is removed, and less is replaced if one thinks it desirable to have some protection in the vaginal vault.

Ries does not use any binder and yet he has not observed a single hernia except in one instance of an infected wound. I confess that I have been too timid to take such a chance, feeling safer if the abdomen was immobilized, though it certainly causes more or less discomfort to the patient, especially during the first few days.

The most serious objection which my confrères have uttered against the early getting out of bed is the danger of thrombosis and embolism, especially after operations for myofibromata. I say most emphatically that this protest is based only upon theory, so far as my experience goes. Thrombosis and embolism occur not infrequently after abdominal operations when the sacred plan of rest in bed for three or four weeks is adhered to. It occcurs occasionally even without any operation, especially in fibroids. It is an unfortunate result of this form of neoplasms, which not infrequently cause cardiac changes. I have not seen a single instance, up to the present time, following my myoma operations with the plan of treatment advocated, and yet some of the myofibromata removed have been very large. Pelvic elevation during operations is far more likely to cause undesirable results, fortunately not permanent, than the early getting patients out of bed after laparotomies.

According[1] to Albanus, there are four factors which may play some part in the etiology of thrombosis: "First, any disturbance of the venous circulation which might exist before the operation and would predispose toward thrombosis, as, for example, heart lesions, varicose veins, exhaustion, prolonged decubitus, and pressure of abdominal tumors. Secondly, conditions attending operations, such as unavoidable chilling and exposure of the contents of the abdominal cavity and possibly traumatism to the vessel walls. Thirdly, the injurious effects upon the heart muscle of the anesthesia, and lastly, topographic relations of the vessels. It is readily understood how a distended and overloaded sigmoid flexure might make pressure upon the left iliac vein. Riedel and Gerhardt call attention to the pressure exerted by the iliac artery upon the iliac vein."

"Clark, after careful investigation based upon clinical and experimental observations, has come to the conclusion

[1] Quoted from Keen's Surgery. Chapter on Post-operative Thrombosis.

that a post-operative thrombosis is due to the effect of traumatism exerted upon the deep epigastric arteries during the course of the operation. The primary thrombosis originates in the deep epigastric vein and is slowly propagated along the line of the vessel until it reaches the external iliac vessel, where it gives rise to a retrogressive thrombus in the femoral vein. 'That the thrombus appears most frequently on the left side is attributed to the mechanical conditions on the left side which retard and derange the femoral and iliac circulation.' Human thrombosis is often caused by the liberation of fibrin ferment in the general blood stream, and chemical changes of the blood have likewise an influence on the occurrence of thrombosis. The precise effect of these chemical changes is as yet hypothetic."

Whether the opening of the peritoneal cavity brings about any of these factors, is, I believe, still an open question.

The generally accepted view of the cause of post-operative thrombosis is that it is due to infection during an operation. While this may be admitted to be correct in many instances, it is my firm belief that it is erroneous so frequently to attribute a thrombosis to an infection. For instance, the only case of phlebitis that I have observed after the early getting out of bed after an operation was that of a young lady, sixteen years old, upon whom I did an abdominal section for the removal of an ovarian tumor on one side, and the exsection of an ovarian cyst on the opposite side, in which a goodly part of ovarian tissue could be left. It was an absolutely clean operation and the patient was out of bed fourteen hours later. The convalescence was perfect, yet a few days after she had gone to the country a mild phlebitis of the external saphenous vein showed itself, which lasted nearly two weeks.

This case convinced me that there were factors which caused circulatory disturbances that were not yet clear to us. Because, here we had an instance in which no infection

took place, and, further, in which rest in bed was neither insisted upon nor taken. I believe that the occurrence of thrombosis may occasionally be attributed to circulatory disturbances brought about by enforced absolute quiet in bed, because inactivity of the muscles and diminished propelling power of the heart causes retardation of the circulation. This would be obviated to a great extent if inactivity were not insisted upon. *There is no better way of keeping up a good circulation than by taking rational exercise.* The fact that thrombosis, if unilateral, has usually been observed on the left side may probably be accounted for by the greater length and the course and relations of the left iliac vein.

Being desirous to obtain some statistical data on the frequency of the occurrence of phlebitis, thrombosis, embolism, etc., I wrote to a few of my colleagues, and here express most grateful appreciation to those who were good enough to answer my request. In addition to the statistical report, Dr. William J. Mayo, with whose permission I quote from his letter, wrote:

"We have come to the conclusion that the blood changes which follow the opening of the peritoneal cavity increase the tendency to clotting and that the muscular inaction due to the lying in bed with the change in the blood pressure, and the interference with the circulation on the left side, are the main causes. We found that patients who got up within a week, like chronic appendicitis, did not have phlebitis, as it usually comes on between the tenth and twentieth days, so that we now bend every effort to get our patients about early and encourage movements of the limbs, especially of the left, with patients who lie in bed a longer time."

Dr. William J. and Dr. Charles H. Mayo, during 1904, in 1788 abdominal operations, had about 1 per cent. of left-side phlebitis. Many of these were mild. For the last year and a half, since they have not enjoined bed rest so strictly, but have endeavored to get their patients about as soon as possible, they have seen it very rarely, not to exceed more than one-third of 1 per cent. In 1905 they had in 2157 abdominal operations, 2 deaths from embolism

One was after an operation on the gall-bladder, and the other a hysterectomy, and also 2 pulmonary embolus cases in which the patients recovered. Dr. William J. Mayo further wrote:

> The more feeble and rapid the action of the heart after the accident, the better the prospects of recovery. In each case that recovered, the heart's action became almost uncountable, the right and the left heart actions not being together, and by its feebleness enabling the lungs to gain a little more time to adjust themselves. The cases that died continued from stronger cardiac contraction to force the blood into the lungs. All of these cases took place between the fifth and the ninth days. In 3000 celiotomies Clark[1] found the record of 35 cases of thrombosis. Schenck, in 7130 gynecological operations, found 48 cases; Albanus, in 1140 laparotomies, 53 cases, and of this number 26 were nonseptic cases.

As to the statistics of thrombosis, etc., in abdominal section patients who were permitted to arise very early from bed:

Dr. Ries informs me, under date of November 14, 1906, that he had one instance of thrombosis or phlebitis with a temperature up to 101.6° F., in a cachectic patient, sixty-six years old, who had cancer of the uterine corpus, with bronchitis, varicose veins of the legs, and arteriosclerosis. In seven days the edema of the leg had disappeared and the temperature was down to 99° F. She spent most of the time in a chair while the thrombosis lasted. Nineteen days after the operation she could leave for home. Last year a thrombosis developed ten days after a plastic operation for descensus of the vagina and uterus, the patient having been up before the development of the thrombosis. It ran the usual course without any symptoms except pain and edema. He thinks that he had another case of mild thrombosis some years ago, but does not remember the details.

Dr. George Chandler, of Kingston, New York, on November 21, 1906, wrote me that he had had 94 cases since February 1 of this year, and in all but 6 cases he had practically, or to a very large extent, followed the treatment

[1] Keen's Surgery.

outlined, and he had not had a single instance of thrombosis or embolism. Dr. Chandler informed me that he let his patients come out of the anesthetic in the sitting position and found less vomit ng as the result.

I have seen only one instance of very mild phlebitis among the entire number of cases in which I permitted the patients to get up very early after an operation. On the other I hand, have had four cases of thrombosis, during the same period, among those who were not allowed to get out of bed soon after an operation, and who were in consequence of the occurrence of the thrombosis kept in bed until all or nearly all symptoms of thrombosis had disappeared.

Undoubtedly in one of these cases, that of a large myoma complicating pregnancy and causing symptoms of such a character as to indicate surgical intervention, the thrombosis would have been attributed to the early getting up, by antagonists of the method, if that course had been followed; but because I was enabled to enucleate by myomectomy the large tumor which was the indication for an operation, and because I was desirous of preserving the conception product, I kept the patient very quiet in bed, and ordered morphine to be given if pain occurred. On the fourth day thrombosis of the femoral veins developed, which kept the patient abed for four weeks. She did not abort.

In another instance of a complicated hysterectomy for a retroperitoneal fibromyoma, in a patient who came to me from Illinois, and who was operated upon two days after her journey, she had such a poor pulse that I did not deem it advisable to follow my usual plan of treatment, because her pulse continued to be of such quality that it seemed too risky to allow her to move about. By the end of the first week she had a double phlebitis from which it took her nearly four weeks to recover. If the woman had got out of bed on the third or fourth day, and this had occurred, undoubtedly antagonists of the method would have held that responsible, whereas, as a matter of fact, the neoplasm

had primarily caused circulatory disturbances, and very probably, had I not been too timid, but despite the pulse had taken my patient out of bed and given her moderate exercise, it might have been avoided.

My associate in the medical school, Dr. A. Brothers, had a number of laparatomies in which he, to a large extent, followed the line of treatment advocated by me, and he also has not seen any bad effects in the circulatory system or elsewhere.

It seems to me that such experience should be quite conclusive proof that the treatment, to say the least, is not contributory to the production of circulatory disturbances, even if it is not to some extent a prophylactic.

The question of diet, which is so much dwelt upon, I discarded, or rather revised, in my practice nearly twenty years ago. It came about by what may be called an accident. On November 17, 1886, I operated upon a woman with double gonorrheal pyosalpinx. On the third day after the operation she had been seized with all the subjective and objective symptoms of peritonitis, and nothing in the way of nourishment was retained by her. She consequently also suffered intensely from thirst. She begged me for permission to drink a bottle of beer. Believing that she would die and that nothing further could be done for her, I did not feel inclined to thwart her and therefore acquiesced in her wish. Lo and behold, she began to retain nourishment, and from that time on she made an uninterrupted recovery. While this was of no scientific value, yet the incident gave me much thought, and gradually I began to change my opinion so far as diet was concerned, so that since some time in 1887 I have been so far settled on this question by experimentation and observation that I have permitted regular diet after abdominal operations after twenty-four hours, unless special contraindications existed.

What are the advantages of the nonrestriction in diet and

moving about at random, and the inducement to have patients move about and get out of bed soon after abdominal operations, unless there are some very decided reasons to contraindicate this plan of treatment? I answer the question only from my observations and personal experience, empirical if you please.

It is important to take a series of complicated operations, treated on the plan suggested, and compare that series with an equal number of patients with about similar complications, treated by the generally approved method. I have gained from such a comparison the impression that the mortality rate is lower in the first class. This, of course, I am not in a position to prove; it is simply an impression gained by observation. *Next there is less nausea and vomiting, also less abdominal distention, because flatus is passed more readily when patients are sitting up. Spontaneous action of the bowels occurs earlier. There is less liability to bronchial and pulmonary complications. There is less liability to circulatory disturbances. There is better assimilation of food. There is less weakening of the general physical condition of the patient. In short, there is more rapid return to working ability.*

What are the disadvantages? If the case is at all suitable for the treatment, *there are no disadvantages whatever, so far as my experience goes.*

The gentleman who opposed me in the paper already referred to, in the subsequent discussion suggested the matter of pecuniary gain—I think thoughtlessly or unintentionally—as one of the disadvantages. Those who have only the mercantile view in mind should not choose the method of treatment advocated, but those who practise their profession as they should will find that it has advantages. We should make our vocation a benefaction to the physical condition of mankind and endeavor to progress in knowledge. To do this, we should not cast aside methods of treatment which do not immediately appeal to

us as good, especially when others, who have had large experience, have reported favorably upon them, until we have proved that they possess no merit, or are perhaps harmful.

Until recently I did not permit patients, upon whom a ventral fixation or a ventral suspension was done, to rise before the termination of the second week, but, in a number of patients lately operated upon, who were allowed to get up and go about in the same way as other abdominal section patients, examination did not indicate that the early moving about had any deleterious effect. Still, this is a point upon which I am not ready to express an opinion until I have had more experience.

Conclusions.—No particular preparatory treatment is necessary for patients upon whom it is intended to do an abdominal operation, unless the operation involves the opening of the stomach or the bowels.

Stomach lavage is of benefit at the conclusion of the operation.

Patients should not be kept unnecessarily under an anesthetic.

The application of a tight bandage around the upper part of the thighs, to keep a blood reservoir in the lower extremities, in exsanguinated and very weak patients, is excellent. The same may, in exceptional cases, be done with one of the upper extremities. These bandages are taken off as soon as the operation has been completed, and thus more blood is thrown into the trunk.

The administration of strychnine during and after an operation should be attended with more care than is usually the case.

The intravenous infusion of a 0.9 per cent. saline solution should not be too long delayed when the condition of the patient indicates that its employment may be of benefit. In instances of large myomata, where the patient has been much exsanguinated by bleeding, it is desirable that the infusion be begun as soon as the patient is fully under

an anesthetic, so that by the time the operation has been completed, about 1000 to 1500 c.c. may have been infused.

The application of a very simple dressing over the wound, and the adjustment of a snugly-fitting Scultetus bandage made of oxide of zinc plaster.

The administration of a dose of morphine if restlessness or pain makes this desirable, the medication then, clinically, acting as a heart stimulant.

The allowing of regular diet and unrestricted mobility within twenty-four hours after the operation, unless specially contraindicated.

The getting of patients out of bed as soon as possible after an operation.

The avoidance of forced catharsis before the first four or five days after an operation, unless there is a special indication for it.

In instances where resort to vaginal drainage is had, or where it is evident that there will be some intraperitoneal secretion after an operation (purulent cases and oozing from torn adhesions), the employment of trunk elevation as soon as the patient is put into bed. For this the employment of a bed-lifter such as described, or the placing of high blocks or chairs under the head of the bed, is preferable to back rests.

## DISCUSSION.

Dr. Henry T. Byford, of Chicago.—Dr. Boldt is one of my good friends, but when I have my first laparotomy performed I will not call on him to do it—unless he will agree to give me a rest after it. I would rather lie in bed for a week or ten days after an operation even if I did not want a rest, than to be put in a strait-jacket and made to go about in it for four weeks. But, I would undoubtedly wish to rest in bed for a week after the pathological conditions and preparations leading up to a laparotomy, and the sickness and dieting follow the anesthesia. The majority of patients requiring an operation are benefited by such a rest without reference

to post-operative customs, and the rest and regulated diet often constitute a part of the cure. Again, I would not like to have my wound closed on and off for a few weeks. I would rather have it examined at the end of a week.

Of course, there are one or two disadvantages in allowing patients to remain in bed a week or ten days after operations. One is that the bowels and bladder may become atonic. But, if the bladder should become atonic the patient can be raised to a sitting position for urination, and laxatives not only restore peristalsis, but they prevent the formation of intestinal adhesions.

Dr. John B. Deaver, of Philadelphia.—I have been much interested in the strenuous recommendations and strenuous treatment of the essayist. I believe there is a good deal in it. Since I have been allowing my patients to get out of bed earlier than formerly, and have allowed them to turn over on their side immediately, after coming out from under the influence of the anesthetic, I do not see as many cases of phlebitis as I formerly saw. Remember, I am not speaking now of septic cases; I mean plain cases. Since I have been allowing my plain cases to sit up in bed the day following an operation, and keep them sitting up, I do not see any cases of pneumonia. So there is reason in the recommendations of Dr. Boldt, and while we may feel a little skeptical relative to the results from pursuing this course, I am strongly impressed with the fact, concerning which there can be no question at all in my mind, that thrombosis, phlebitis, and embolism will become less and less from this method. I have seen cases of embolism in patients who were kept in bed for three weeks, but I can not recall, since my practice has been more radical, any case of embolism having occurred, and I am therefore willing to endorse the method of Dr. Boldt to a certain extent.

Dr. Rufus B. Hall, of Cincinnati.—There is a great deal of difference in the condition of patients after abdominal section, particularly after a severe operation, and while a patient may be allowed to turn over on the side after an appendectomy and throw the legs and arms around, but get up and be permitted to sit in the chair or walk around twenty-four hours after operation, I would not be willing to give my consent to this mode of treatment. While Dr. Boldt's patients undoubtedly recover just as he says they do, I believe they would recover just as comfortably and more so with the

ordinary after-treatment, than to allow them to sit up and move around at the end of the first or second day. I do not think we should allow what he has said to go unchallenged, as good treatment after abdominal sections as they ordinarily go. There is a middle ground that we ought to take. There are many patients to whom this after-treatment would do harm. In other cases it might be good treatment, but, all things considered, I cannot endorse the method; I do not believe it is correct. It is a very different thing when we have cases of enlarged prostate to deal with and have them sit up after operation. It would be good treatment in such cases. If I were to have my prostate removed I think I would like to sit up as soon after the operation as possible, on account of drainage, but in a case of appendectomy I would prefer to stay in bed for a few days.

Dr. J. Montgomery Baldy, of Philadelphia.—We can tell any patient after an abdominal section to turn over on her side, but she will turn over on her back again very quickly. She will not wait to be asked to do so. There is reason for that and the patient quickly finds she is more comfortable on her back than on her side no matter how uncomfortable she is in the recumbent dorsal position. It must be very disagreeable—yes, horrible—to take a patient twenty-four hours after a severe abdominal section and force her to sit up whether she is in a straight-jacket or not. Dr. Boldt claims, for instance, that the patient's digestion is very much aided, and she can be given a more liberal diet. In my experience I find that these patients after operation have very little desire to eat even if you give them food. Some of them will not eat. It is the hardest thing to get them to eat much food before the end of four days and there is no particular reason why they should.

Let us take a patient, for instance, who has been an invalid for a month or six months; she undergoes a severe operation and a few hours after it she is forced unto her feet; to me the proposition is outrageous; it is unreasonable. Such patients are sick without operation, and they are the ones who need rest in bed after operation. Please remember, I am not talking about simple cases of appendectomy; cases of appendiceal colic; cases of oöphoritis, with punctured cysts, etc., as women with such ailments go to bed in a fairly good physical condition after operation; but, I am talking about

the suppurative cases of appendectomy; suppurating gall-bladder cases, etc. It seems to me unreasonable to put such women on their feet inside of twelve or twenty-four hours after operation.

So far as the bowels are concerned, you can open them in twelve or twenty-four hours after operation. They cannot be opened more quickly by nature; they can be opened with a slight amount of laxative. I do not see any particular advantage to be gained.

I can not agree with a single statement that Dr. Boldt made relative to the advantages he claims in getting patients on their feet in twenty-four hours after operation. The evidence he furnishes does not satisfy me. If I turn to the only place I can turn for evidence, namely, to my own experience, it is completely and absolutely to the contrary. Understand, I do not take issue with those men who allow their patient to get up and go about at the end of ten days or two weeks. Personally, I believe in keeping them in bed a longer period after operations. I usually keep them in bed three weeks. I do it for the sake of giving them the rest they need. They should be given a good, careful, nutritious diet, careful nursing, pleasant surroundings, and kept away from the petty worry of members of the family, husbands, babies, etc. A large number of patients are benefited by the rest and proper treatment which they receive in bed after operations. As I told Dr. Polk many years ago, when gynecologists were resecting ovaries and bisecting tubes, many of these patients, by proper rest in bed and careful nursing, if taken away from their homes and from the worry of their families, would have been cured *minus* the surgery.

Dr. Howard A. Kelly, of Baltimore.—This is a very important subject. It is not like the question of one man preferring one operation and another man preferring another, because if Dr. Boldt's plan is carried out it will multiply the facilities of all our hospitals two or threefold and be a saving of millions of dollars. If the plan should be very generally employed it will mean an enormous increase in our hospital facilities. I am impressed with it, and if I can overcome the conservatism of my associates I shall carry it out.

I have been interested in listening to what was really a tribute to Professor Lenander, of Upsala, who has long insisted that most of our cases of post-operative phlebitis occurs

from stasis in the vessels, due largely to the absolute rest in bed after operations, but possibly associated with some traumatism. If patients are given more exercise after operations, I can readily see how we will have less phlebitis. This applies particularly, I think, to hospital cases. In our private hospitals we get patients from the higher walks of life who are exceedingly nervous, and they are more apt to need a prolonged rest in bed after operations.

Dr. Joseph Price, of Philadelphia.—I shall not try the plan suggested and recommended by Dr. Boldt. I feel like giving my patients another week in bed. I am glad, however, Dr. Baldy made the splendid discussion he did. Most of you have undoubtedly read Hilton on *Rest and Pain*, a book that has attracted the attention of the world. It has done surgeons good. The neurologists, the rest-cure men, ought to be here to take up this subject and tell Dr. Boldt a few things about the importance of rest, and what they have accomplished in emaciated, anemic, and exhausted women. I refer to the Weir Mitchell treatment, for instance. Our patients, many of them, have exaggerated forms of the trouble that patients have who seek the Weir Mitchell treatment. They are less strong, more anemic; the contraindications against early rising after operations are stronger in our group of cases than in those coming to the neurologist for the rest treatment. We accomplish thrice more than they do. We deal with septic forms of disease, with suppurative forms of disease which extend to the iliopectineal line or umbilicus. Anything in the germ zone below the umbilicus we deal with surgically.

I saw a handsome boy a few weeks ago who was greatly emaciated from a short attack of appendicitis. He came to me; I examined him carefully and diagnosed appendicitis. He wanted to know if he could go home. I said to him, "My lad, you can't go home; your appendix must be removed this afternoon." There was only one argument that influenced me, and that was, he said he wanted to go home to see his mother. She was an invalid. While the boy was up and about, he was very thin, and if operated on should be kept in bed four weeks thereafter. Nearly all of the operations on the class of cases under discussion are successful, particularly when done in a well-managed, well-equipped hospital. In these institutions you have opportunities which you should

always take advantage of. Very few hospitals in this country are well run. The old hospital was the pest-house, and it was the private hospital that improved the general hospital. It was the private hospital that patients had to go to rid us of the old general hospital, the pest-house. It has done its work well.

There is nothing in the paper that commends itself to me. Dr. Baldy has said enough on this subject to demonstrate very well my own position in this matter.

I have alluded to the emaciated condition of these patients; to the septic conditions. Many years ago, Dr. Goodell removed an ovarian cyst from a woman residing in Pottsville, Pa. She remained in bed about three weeks and made a good recovery. On her way home from the hospital the car got off the track, bumped along the ties, the incision parted, and her viscera protruded onto her petticoats. Some four years later I assisted Dr. Deaver's brother, a good surgeon, in Germantown Hospital, in the removal of an ovarian cyst. The abdomen in this case was greatly attenuated. The operation was done satisfactorily; convalescence was a speedy one, and at the end of four weeks the woman walked home. That night there was a fire next door; a general fire alarm was given; the woman sprang out of bed, and her incision opened from end to end. I could cite a number of other instances of accidents of that nature, due largely to early rising after operations.

Every nurse in a hospital should be a good rubber. They should all rub well. If you go into a certain hospital at 8.30 P.M., you will find that patients are well rubbed and well pounded by the nurses. This rubbing is a good thing for patients.

With regard to putting these patients on an increasing suitable diet after operation, it seems to me it is folly to talk about feeding them pork and cabbage when they will not take it, and if they do take it it will be vomited, and nausea and vomiting are harmful after these operations.

In brief, I do not see how any good can come from a paper of this character. It is my impression that these patients should remain in bed even four weeks, and not three, after major operations.

DR. P. F. CHAMBERS, of New York City.—All of us cannot fully endorse what Dr. Boldt has said with reference to

allowing patients to get out of bed so soon after operations; nor can we endorse the practice of Dr. Price of keeping every patient in bed four weeks after operation. I do not think any hard and fast rules can be laid down for the after-treatment of these patients. I have had many patients where I thought it was perfectly safe for them to sit up on the third or fourth day after operation; while in others I have thought it necessary to keep them in bed for three or four weeks. Each patient has to come under the direct observation of the surgeon, and is to be governed by not only her previous condition, but her condition after operation, as to when he will allow her to get out of bed.

Dr. M. C. McGannon, of Nashville, Tenn.—It behooves every man here, who is doing abdominal surgery, to consider this radical departure from a usual custom and voice his sentiments upon it. I fail to understand what induced Dr. Boldt to adopt this method. What accidents have occurred in his particular work to induce him to make this one radical change, viz., getting his patients up so early after operations. There is nothing else in the paper that is in any sense of the word new. The practice of giving patients opiates for relief of pain; of leaving the bowels quiescent for days after the operation, etc., are now and have been for years adopted by many of us. But, this particular idea of getting patients out of bed so early is a thing I would like to have some explanation about, especially as to the original cause which induced the essayist to adopt it.

The mechanical proposition of dressing a patient in such fashion that she may get up at the end of twenty-four hours is quite feasible, since the abdominal wound may be closed in a way that, even without this bandage to which Dr. Boldt has referred, the patient may be permitted to get up and still do no damage to the wound. I believe, too, we may put on such a bandage as he has described, or one of plaster of Paris, and allow the patient to get out of bed without much danger. There are many things to be considered beside the mere fact of allowing patients to get out of bed soon after an operation. What are its advantages and what are its disadvantages?

The essayist has spoken of phlebitis, pneumonia, and thrombus as though they might be prevented by the position.

Phlebitis is not a common sequela of uncomplicated abdomi-

nal section and in no case can I conceive of its being due to the prone position, or of its being prevented by encouraging the patient to assume the upright position. As a matter of fact, when it does occur the upright position is the last a surgeon would adopt for its relief.

I have not heard before that the prone position was a etiological factor in the production of pneumonia, nor can I conceive of it in any way so influencing the lung tissue as to make it more liable to pneumococcis invasion.

Thrombus is not a very common complication after abdominal section, even when done for myomata of the uterus. Those of us who work in the South, where our hospital patients are largely of the colored race, have greater opportunities of observing the results of operations upon fibroid uterine growths than operators have in Northern fields, where the negroes are few. In my work I have had practically no cases of thrombus and I am sure my experience is not unique. Let us, however, for the sake of argument admit that it arises often; then we must also admit, from the very circumstances, it must occur almost immediately after the operation and could not be influenced favorably by sitting the patient up twelve or fwenty-four hours subsequently. It might be unfavorably affected by the procedure, since the clot could thus be loosened and carried on into the circulation to produce disastrous effects in some distant part of the body.

As I understand Dr. Boldt this bandage, upon which he lays stress as an essential factor in the early getting patients out of bed, is purely a mechanical aid to prohibit the active contraction of the muscles, support the patient, and thus prevent pain. While it will accomplish these results it will also tend to cause atrophy of the muscles and thus, instead of hastening recovery, will retard it.

I might also invite your attention to the fact, that, probably, all of us have had experience with this method of getting patients into a sitting or upright position after abdominal sections, so that it is really not new. Many of you will recall cases of incurable abdominal diseases and also cases of abdominal difficulties in old people, when (after abdominal section) for some reason you wished them to be out of bed, you have applied a strong abdominal supporter of some kind (plaster-of-Paris or adhesive plaster) and gotten them upon their feet. I doubt if you have been impressed with any advantage

to the patient from the procedure. I believe that we should keep these patients in bed for a proper length of time after an operation, and the length of time should be governed by the judgment of the surgeon. There is undoubtedly a middle ground for us to take in regard to this matter. We may keep patients too long in bed after operations or we may allow them to get up too early. Above all things, we should keep patients in bed a reasonable length of time after an operation, according to the nature of the case, and we should have definite reasons for our course; reasons based upon sound and logical physiological and pathological facts.

Dr. Abram Brothers, of New York City (by invitation).—I think the paper read by Dr. Boldt is one which marks a new era in the after-treatment of our surgical cases. No new movement has ever been started without meeting with violent opposition, and usually the opposition is founded on theoretical consideration. The gentlemen who have opposed this new method of treatment tell us they have never seen it tried, and have never tried it themselves, and some do not see the necessity for trying it. This recalls vividly to my mind something which came from Baltimore a few years ago when Clark promulgated the idea that all kinds of pus which soiled the peritoneum were not necessarily fatal; that it was not absolutely necessary to flush out the peritoneal cavity with pailfuls of water because a few drops of pus entered the cavity; nor was it necessary to put in drainage tubes or to use gauze drainage to aid in the recovery of the patient. I was one of the first workers in that direction, being satisfied with the method of Clark, of closing the incision, after wiping away a little pus in the peritoneum which had escaped, and found that patients underwent a much better convalescence than when the peritoneal cavity was flushed or washed out with large quantities of water. In referring to this, at a medical society meeting, one of the members said, "If I were to have my belly cut open, and there was pus present, I would not let Dr. Brothers do so." Now, there is not a gentleman in this room, or a member of this Association, who does not concede that there are cases in which there is pus in the peritoneal cavity, particularly in cases of pus tubes, and in these cases the surgeon has no hesitation in removing the pus tubes and closing the abdominal cavity without drainage, and there can be no question but what the results are not only

as good, but better. But that does not deny the proposition that drainage is still employed. It simply proves that we get good results by recognizing the fact that the peritoneal cavity has a certain absorptive power of its own.

When this new proposition was brought to my attention two years ago, by my respected friend Dr. Boldt, it sent a thrill to my brain, as it has done to many others here today. There is not a gentleman here who felt a greater thrill of horror pass through him at the seemingly brutal idea of having a patient, after a major operation, sit up within twelve or at least twenty-four hours for, say, fifteen minutes; then putting the woman back to bed and repeating this on a larger scale the following day. However, after seeing the results of Dr. Boldt's work, this thrill gradually passed away. I found that these patients did better; they were eating more heartily; their morale was changed. The great dread and fear with which they approached the operation disappeared when they found they were able to sit up in a chair; and many of the symptoms we meet with in abdominal work, such as excessive vomiting, great distention of the abdomen, and excessive pressure, were relieved when the patient was allowed to assume the upright position. When these things began to dawn on me, I tried the method in many cases. I thought that Dr. Boldt was an extremist in that he allowed some of his patients, after operation, to sit up in twelve hours. When I observed the results from this plan of after-treatment, I said to myself that it is not right for us to keep the patients in bed three weeks after operation. There is a midway, however, between the two extremes, and which I believe is the proper course of treatment of cases in the future. I think this Association, as well as the profession, should be very much indebted to Dr. Boldt for having brought this subject so clearly before us. While it may not revolutionize our previous plan of after-treatment of operative cases, still it will materially change and modify the method and manner of convalescence of patients after major operative work.

There are two ways to approach a subject of this kind. Theoretical reasoning is not going to do it. Theoretical reasoning can be answered by arguments that are just as forcible from a practical standpoint. The post-operative dangers of re-opening the abdominal wound is done away with by bandaging. These patients are not put in a plaster-

of-Paris bandage or strait-jacket. I have had 50 of these cases of early mobilization after laparotomy, and I recall only 1 case in which I had to loosen the plaster. These patients are just as comfortable in a plaster bandage as without it. They are just as comfortable as they are with an abdominal bandage. In neither case are they absolutely comfortable. I claim that you can carry out the idea of immobilization very well, and that is what a great many gentlemen seem to forget. It is not a question of straight plaster-of-Paris fixation of the abdomen, but a question of assisting immobilization.

Dr. Henry O. Marcy, of Boston.—Just a word in reference to what has been said in regard to extremists. In the last 2500 laparotomies I have treated the abdominal wound by closure with buried kangaroo-tendon sutures. The edges of the skin are held in coaptation with an aseptic seal of iodoform collodion, reënforced with a few fibers of cotton, no bandage whatever, and the patient is comfortable. There is no strain upon the abdomen unless something unforseen occurs to complicate it, and in every aseptic case we have not had hernia.

Dr. E. A. Balloch, of Washington, D. C.—We have heard this subject discussed from the standpoint of the operator. I propose to say a few words from the patient's point of view. Last May I had a little difficulty with my appendix and instituted divorce proceedings, and underwent operation. I heard Dr. Boldt's paper at Atlanta. I am sorry I heard it because I thought I was going to do just as he described his patients had done, and were doing. I had my stomach washed out after anesthesia, so that didn't bother me after the operation. Some five or six hours following the operation I thought I would like to turn over in bed. I tried it once, but that was enough for me. I did not want to get up in twenty-four or forty-eight hours; I was perfectly content to lie there for a week or more. The chair had no attractions for me.

I want to endorse, however, what Dr. Boldt said with reference to diet. I made up my mind when I went to the hospital that I would not venture any opinions of my own. So I let them purge me and enema me to their heart's content. But after the operation I did not have any nausea; they began to feed me on egg albumin and broths of one kind and

another, and the more I partook of that sort of stuff the more gas I got in my intestines. Finally, on the second day, I said to my surgeon, "Look here, I am perfectly willing for them to give me nothing at all; I can starve comfortably, but I do object to taking any more of this sort of stuff." He said to the nurse, "Let the patient have his own way; give him what he wants." I told the nurse to send me up all they had. They sent me up a half of a baked chicken, toast, salad, and tea. I ate the whole business. (Laughter.) I was hungry. I just made up my mind that if I died I was going to die happy. I was not feeling very well with all of this gas on my stomach, but this disappeared and in ten minutes I felt good. I do not know whether it was imagination or not, but I felt perfectly happy after partaking of this meal. From that time on I had very little discomfort. I did not want to get up in twenty-four hours, however, although I thought I was going to do as Dr. Boldt said his patients did when he read his paper at Atlanta. He has his patients well trained, or I did not have the courage or moral support of my surgeon. A week in bed was good enough for me. I went home at the end of ten days and even then I thought I would have felt much better if I had remained a week longer.

In conclusion, all I have to say is that the standpoint of the surgeon is one thing, while that of the patient is entirely another.

Dr. Boldt (closing).—I have been dealt with rather leniently, but that does not change my point of view.

With reference to the remarks made by Dr. Byford, I will say that he is not carrying out the customary plan of after-treatment if he allows his patients to be up in a week. That is a decided step forward. If anyone is getting his patients up and about at the end of a week's time he is certainly not employing the old after-treatment. The old plan of after-treatment was to keep patients in bed two or three weeks or longer.

So far as the diet is concerned, I have distinctly stated that if there are no contraindications a regular diet is prescribed. I maintain, however, that we cannot treat eighty or ninety patients in the same way. There must necessarily be some variation in the after-treatment.

In reply to the remarks made by my friend, Dr. Price, I believe it would do harm to take a neurasthenic out of bed

early after operation. It should not be done. There are exceptions to be made and, as I said in my paper, much more discrimination is required in adopting the plan I have recommended than in carrying out the old-fashioned method.

There is not so great a difference between mobility in bed and allowing patients to sit up, as remarked by Dr. Hall. It is practically the same thing whether you let them turn in bed or move their legs. The patients usually are better satisfied than when they are kept in bed.

In reply to the remarks of Dr. McGannon as to why I adopted this method, my direct reason was this: Dr. Ries, of Chicago, I heard, allowed his patients to get up early after laparotomy. I communicated with him and he told me that I was mistaken in that he allowed his patients to get out of bed three or four days after operation, for really he allowed them to get up the first day. I was more readily converted to this view, because of the large number of vaginal hysterectomy patients whom, for many years, I had gotten out of bed within twenty-four hours after operation.

I would like to ask Dr. Balloch whether he had a regular oxide of zinc Scultetus bandage on or not?

Dr. Balloch.—No; plaster-of-Paris bandage.

Dr. Boldt.—There is a great deal of difference between a carefully applied and snugly adjusted oxide of zinc Scultetus bandage and plaster. It gives them freedom. I do not restrict my patients to broths and peptonized milk when they want something else. These patients are allowed to move about and to do as they please.

# THE TREATMENT OF SENILE GANGRENE.

By Edward H. Ochsner, M.D.,
*Chicago.*

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At various periods in the past there has been considerable difference of opinion as to the time when amputation should be resorted to in senile gangrene, and even now, if one will look over the literature on this subject, he finds almost every conceivable attitude taken in reference to this matter. As long ago as 1884, Jonathan Hutchinson, in his masterly article,[1] advised early high amputation in all cases of senile gangrene. At that time this advice was undoubtedly most excellent, but surgery has in the meantime made such tremendous strides and conditions have changed so markedly that the same rule no longer applies. Besides, I believe I will be able to show that there are two distinct types of senile gangrene, and that these two types should be clearly differentiated. When this is done, I believe it will be relatively easy to determine whether early or late, high or low amputation be indicated in any individual case.

If the term senile gangrene is limited to mean that form of gangrene which is due to arteriosclerosis, as I think it should be, one can distinguish two primary classes, both from a pathological standpoint and for the purpose of rational treatment: First, a senile gangrene in which there may be, and usually is, a more or less generalized arteriosclerosis, but in which the lumen of a certain artery supplying the distal portion of an extremity suddenly becomes markedly

[1] Med.-Chir. Trans., 1884, vol. lxviii, p. 97.

Fig. I

Fig. II

Fig. III

constricted at a single point by a mural thrombus so as to cause complete or almost complete obliteration of the lumen, so that the circulation distal to this point of constriction becomes distinctly insufficient to nourish the part, while the artery proximal to the above point of constriction remains sufficiently patulous to insure reasonably good nutrition to the tissues supplied by it.

This form of senile gangrene corresponds in its symptomatology perfectly with the cases described by Bilroth[1] under the heading, Spontaneous Gangrene, but by him differentiated from senile gangrene because his cases occurred in patients with embolism due to endocardial vegetations.

Second, a gangrene due to general arteriosclerosis, in which the lumen of the vessel becomes progressively smaller and with it the nutrition of the limb becomes poorer and poorer, the farther distally one goes. The vitality of the proximal portion of the limb is only slightly impaired, but this becomes more and more pronounced the farther one progresses distally until a point is reached where the red streaks appear, then brownish discolorations follow, which finally merge into the gangrenous area.

In order to make myself more clear, I have had a semi-diagramatic drawing made of the longitudinal section of a normal posterior tibial artery, another in which the lumen of the vessel becomes progressively smaller, and one showing a mural thrombus causing almost complete obliteration.

Fig. 3 is adapted from Spalteholz and Schæfer, and in it the thicker portion of the wall consists of the media and adventitia, while the endothelial layer and the intima furnish only a very small portion of the thickness of the vessel wall. In Figs. 1 and 2 the media and adventitia are represented as unchanged, because ordinarily the greatest change occurs in the endothelial layer and the intima. In Fig. 2 the thickness of these two layers is more evenly distributed, and as the lumen of the vessel becomes smaller this thickening

[1] Billroth, T. Clin. Chir., Wien, 1871, p. 515.

encroaches upon it until it becomes so reduced in caliber that the artery is no longer able to supply the member with sufficient blood. In Fig. 1 this change is represented as occurring more markedly in one place as a mural thrombus.

I wish to emphasize that the sketch is only semidiagramatic, especially in reference to the difference in the condition of the branches of the posterior tibial arteries in Figs. 1 and 2. In Fig. 1 the lumen of these branches is represented as not deviating markedly from the normal, while in Fig. 2 the lumen of the proximal branches is considerably encroached upon, while the distal ones are entirely obliterated. I have not had an opportunity to demonstrate this to my entire satisfaction, because it is extremely difficult to trace these branches, and the specimens that have been at my disposal have been very limited in number; but so far as my observation goes in this respect it is as represented by the figures, and I believe that, clinically, the presence or absence of collateral circulation has much to do with the progress of the disease and with its clinical manifestations.

Of course there are all shades of transition between these two primary classes, and yet the two types are sufficiently distinct to be worthy of differentiation, especially as the recognition of these differences points the way to the successful treatment of senile gangrene.

That these two classes of cases actually occur in practice I have been able to demonstrate on several occasions. I have had an opportunity to examine two amputated lower extremities, amputated high, when there had been a distinct line of demarcation. In these cases I found the vessel of reasonably good caliber until a marked constriction was reached several inches above the line of demarcation. The second type is well illustrated by the third history which I give below.

In both forms of senile gangrene it is very important to protect the part against unnecessary traumatism, undue pressure, excessive variations in temperature, and, above all,

infection while waiting for the proper time to operate. This is best accomplished by dressing the limb in dry, sterile gauze and absorbent cotten, having the prominent parts well padded and the bandage loosely but evenly applied.

The first class of cases is more apt to occur in younger individuals with greater general vitality. In these cases the marked narrowing causing the complete occlusion frequently occurs quite suddenly; the line of demarcation forms rapidly and distinctly; the phagocytes are active and the organism often has sufficient vitality to successfully amputate the gangrenous extremity. It takes a considerable amount of energy on the part of the organism to successfully do this, but because of the good general health of these patients one can safely wait until the line of demarcation is distinct and then hasten matters and conserve the energy of the individual by amputating directly through the line of demarcation and allowing the wound to heal by granulation.

As an illustration of this class of cases, permit me to give the essential points in the history of a case which was under our care in 1901 and 1902, and operated upon by my brother, Dr. A. J. Ochsner, April 14, 1902. A man then eighty-two years of age gave the history of having bruised his left great toe on a bath-tub four months previously. A few days later he began to have pain and noticed a dark-red discoloration of the toes extending half way up the foot. The discoloration of the foot gradually subsided, but that of the toes became more pronounced and in the course of a few weeks the toes had become black and gangrenous. Condition on admission, April 13, 1902: Well nourished, general condition good, functions normal, temperature 98°, pulse 70, regular and strong, left foot discolored to level of metatarsophalangeal junction, line of demarcation distinct, several small ulcers on great toe and second toe. The patient was anesthetized, the foot elevated as high as possible, and the toes amputated without an Esmarch, making the skin incision through the line of demarcation and the bones dis-

articulated at the metatarsophalangeal joints. No stitches were applied, the wound was dressed dry and allowed to granulate. The patient left the hospital, cured, June 22, 1902. About two months after leaving the hospital I met him walking without a limp, and though feeble now, is still living and has had no further trouble of this kind.

On August 5, 1906, I saw a patient in consultation with Dr. J. F. Percy and Dr. E. Hertig, of Galesburg, Ill., whose condition I believe is very accurately represented by Fig. 1. He was eighty years of age and when examined was suffering from a gangrene of his little toe on the left foot. He had almost no generalized arteriosclerosis; his left popliteal artery was perfectly soft and the pulse strong; the posterior tibial, as it passes around the internal malleolus, was somewhat arteriosclerotic and the pulse almost imperceptible, and his attending physician stated that several weeks previously the pulse could not be felt at all at this point. There was no cardiac lesion, and this, with the fact that the condition had been about two months in developing, practically excluded the possibility of an embolus. The patient was placed on general tonic treatment; the Bier active elastic constriction of the veins of the leg was continued and in course of time the gangrenous portion of the extremity was exfoliated; the defect healed by cicatrization and epidermization and the patient has made a perfect recovery.

In the second class described, conditions are very different; here the arteriosclerosis is pronounced even high up in the vessel; the greater portion of the extremity may be poorly nourished, so poorly in fact, that in the lower extremity, for instance, the whole leg may be on the verge of gangrene. In these cases there is little or no tendency to the formation of a distinct line of demarcation. Instead, reddish or bluish lines run up the dorsum of the foot and onto the anterior and lateral surfaces of the leg. In addition, the general vitality of the patient is so reduced that it has little or no power to exfoliate the gangrenous member, and in these cases

high amputation should be promptly resorted to before the general strength of the patient has unduly suffered.

Nature's attempt at exfoliation of a gangrenous extremity is accompanied with a great deal of pain, which, plus the actual energy required, takes more strength than this class of patients have at their command, and consequently it is important to amputate just as soon as the surgeon is able to differentiate.

In amputating for this condition there are several points worthy of especial consideration. The operation should be performed without an Esmarch constrictor, because irreparable damage can be done to arteriosclerotic vessels with it. Vessels that might otherwise be able to supply sufficient blood to the part may be so compressed, indented, and damaged by the constrictor as to be unable to satisfactorily perform their function after the constrictor has been removed. Besides this, it is a very severe insult to all the soft tissues and may be just the one thing which will cause the gangrene to spread. The cutting off of the blood supply for ten, fifteen, or twenty minutes may so lower the vitality of the parts as to make it impossible for them to subsequently revive. Instead of using an Esmarch, it is much better to lower the head and body as far as possible and to elevate the extremity which is being operated upon. In this way, the foot, for instance, can easily be held three feet higher than the heart, and if the vessels are grasped quickly as they are cut, an amputation can be made at the point of election in one of these cases without losing more than two ounces of blood. This position during the anesthetic has one other great advantage, it prevents the mucus from being aspirated and materially reduces the danger from post-operative pneumonia.

The amputation must be done sufficiently high. If the incision is made too low it passes through poorly nourished tissue which has no power to regenerate, and in addition to causing the patient the shock of the operation one is likely

to convert a mummified, aseptic extremity into a moist, septic stump, which will tax the patient's strength much more severely than the former condition.

The most painstaking asepsis is necessary. Healthy, vigorous tissue, as we well know, can successfully combat slight infections, while devitalized tissue is practically powerless to do this; hence the necessity of special precaution when operating for senile gangrene.

The operation should be done with as little trauma as possible. The knife should be very sharp and the tissues of the stump should be handled as little and as gently as possible, both during the operation and in the subsequent dressings. The operation should be as rapidly and dexterously performed as is compatible with gentle manipulation; the skin and periosteal flaps should be amply long, the bone being cut very short. The ends of the bone should be covered with periosteum held in place by one or two fine unchromicized catgut stitches. The skin flaps should either be held in place by proper gauze dressings or by only a few silkworm-gut stitches. The drainage must be ample, as nothing is more likely to cause an extension of the gangrene than undue tension from pent-up wound secretions. However, free drainage should be secured by little suturing in preference to any of the drainage devices, because all of these cause more or less pressure and are likely to favor gangrene.

The least possible amount of general anesthetic that will keep the patient comfortably asleep should be administered. I am convinced that this point is of the very greatest importance, as it has much to do with post-operative shock. If the patient is severely shocked for ten to twelve hours following the operation, the parts will not be supplied with sufficient arterial blood, and the gangrene is almost sure to extend. The patient needs all the strength available the first forty-eight hours after the operation, not only to supply the part with sufficient arterial blood, but also that he may

have sufficient strength to sit up in bed with a head rest the day of the operation and be placed in a wheel chair for a short interval twice a day every day thereafter until he is able to walk. The points above enumerated all tend to preserve the general strength of the patient and especially to protect the already devitalized extremity.

In all the articles which I have read it is very evident that a confusion has existed in regard to the treatment of senile gangrene, and that a diversity of opinion has been held and expressed in the past. In none of the articles which I have been able to find has there been a clear differentiation made between the two classes above outlined. In only one was the use of the Esmarch constrictor interdicted, while in several operations reported its use was recommended. Most authors agreed with Jonathan Hutchinson in advising early amputation through the middle of the thigh in all cases, while others favored delay in certain cases, without apparently having a clear conception in which early amputation and in which delay should be practised.

As an example of the above method of treatment, permit me to report the following: Case No. 17,761, male, aged eighty years, tailor, who come under my care February 22, 1906, with a history that about one year ago he began to have pain in the left lower extremity, the pain becoming constantly worse. About six weeks before he came under my care his left leg and foot began to swell and the pain became more intense. Three weeks later, discoloration appeared on his toes and steadily progressed upward. He was otherwise in fairly good general condition, but suffering severely from pain in the left lower extremity. All of the palpable arteries were literally as hard as pipe stems; heart sounds clear; cardiac dulness slightly increased; pulse irregular. Toes of left foot black, gangrenous, with skin abraded in various places; reddish and brownish streaks passing up the dorsum of the foot to a little above the ankle joint; no clear line of demarcation. Four days later the

patient was anesthetized and an amputation through the middle of the leg was performed according to the method above outlined. Barring a small area of necrosis which subsequently had to be skin grafted, the patient made an uneventful recovery.

In connection with this case I would say that I believe in this type of gangrene the amputation must be made at a point sufficiently high in order to reduce to a minimum the possibility of the necessity of a secondary amputation. Just where this point will be will depend upon the individual case, but in most cases a thigh amputation will be required and in that instance I should personally favor a transcondyloid or so-called Carden's operation. The Gritti or Gritti-Stokes operations have never appealed to me in these cases, because I have feared that the nutrition of the patella would be insufficient and I have always tried to eliminate every feature that might interfere with rapid healing.

The one objection to the thigh amputation is that these old patients with thigh amputations have great difficulty in learning to walk with artificial limbs. Besides, many of them are poor and do not feel that they can afford the outlay of an expensive artificial leg. For these reasons it is desirable to make the amputation at the place of election, a hand's breadth below the knee joint, wherever the vitality of the tissues will warrant such a low amputation. Patients over eighty years of age, as the one whose history I gave above, learn to use a peg leg on such an amputation stump very satisfactorily in a few days and derive great comfort from it.

In conclusion, I would again like to emphasize the importance of differentiating between these two primary classes. Though the primary discoloration may be just as extensive in Class 1 as in Class 2 at the onset, the reddish streaks gradually recede after the first few days, and in a pronounced case of the first type a definite line of demarcation forms by the end of two weeks with the skin proximal to this line of demarcation in a reasonably good, healthy, vigorous con-

dition. All the cases in which there is marked arteriosclerosis in the main artery supplying an extremity at a point a considerable distance proximal to the area of gangrene, and in which the discoloration is progressing instead of receding, and in which a fairly definite line of demarcation has not formed at the end of two weeks, should be grouped with Class 2 and an immediate operation resorted to.

By recognizing the difference between these two pathological conditions and adopting the treatment accordingly, one can frequently save a useful extremity, as in the first two cases reported, while in applying the treatment outlined in Class 2 in all cases which do not clearly come under Class 1, can, I am sure, greatly reduce the mortality.

## DISCUSSION.

Dr. Charles H. Mayo, of Rochester, Minn.—I consider this paper a most instructive one. Dr. Ochsner spoke of the Bier congestive hyperemia treatment in some of these cases. It is possible that this treatment in some types of gangrene, with no tendency to demarkation, no line forming, if carefully applied high up on the limb, by using the Martin bandage or the Riva-Rocci air-pressure bandage so as not to induce any undue traumatism to the soft tissues, might be of advantage in forcing the circulation through the venous capillaries, favoring the tendency to repair at some point and in helping to form the line of demarkation. In a case of scleroderma, which I had some years ago, I resorted to amputation of the finger. It was similar to Raynaud's disease, not a symmetrical condition, but a scleroderma which resulted in gangrene, and I amputated the member. The patient came to me four years afterward with the same condition in the next finger. It involved the whole finger. I should say the finger was going through exactly the same process as the other digit went through four years before. I applied the Bier treatment to the wrist twice a day, half an hour at a time, and in five days the attack was over. I believe I should have amputated that finger if I had not tried the Bier treatment and forced the venous circulation into that tissue.

Dr. Henry O. Marcy, of Boston.—Some time since, in Alaska, I saw an original operation by an Indian chief who was supposed to be ninety years old. He had gangrene of one of his large toes. He applied the heroic treatment of chisel and mallet on a block of wood, making a clean cut. He then made a poultice of a variety of herbs, and the toe did very nicely. Flaps to cover the bone were, of course, lacking. Necrosis of a part of the bone followed; granulation and cicatrization completed the process.

Dr. Charles W. Oviatt, of Oshkosh, Wis.—Dr. Ochsner spoke of the advisability of making amputation below the knee. For a number of years I have resorted to the method of amputation known as the Gritti-Stokes, which gives the same result he speaks of in the cases in which an artificial limb is worn. The operation consists of making a supracondyloid division of the femur and a longitudinal section of the patella, fastening it down with silver wire, getting bony union between the freshened patella and the femur. It gives as perfect a stump as we can get in these cases.

Dr. E. A. Balloch, of Washington, D. C.—I want to endorse one thing which the essayist referred to with regard to the use of the Esmarch bandage. I thoroughly agree with him in all that he said as to that. I believe it does more harm than good in these cases, in that it damages the circulation of the vessels and injures the soft tissues, and we get better results when we do not use it than when we do.

Dr. Ochsner (closing).—My reason for writing this paper was because, on this subject, our text-books are very confusing. They make certain general statements, but they do not lay down definite rules which can be adopted in any individual case. I have seen several cases of senile gangrene in consultation with general practitioners and with them, too, this same confusion seemed to exist. It is for this reason that I have taken up this subject in detail.

# TWO SIMPLE SURGICAL TRICKS.

By Ap Morgan Vance, M.D.,
*Louisville.*

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For a long time, in cases of cleft palate, I struggled with the different needles which have, from time to time, been given to us by different operators. I had a fair amount of success with all of them, but none were entirely satisfactory.

Some four or five years ago, while doing a staphyloplasty in the case of a girl, aged fourteen years, I happened, on the spur of the moment, to think of and immediately put into use the following idea in the way of a needle which has since been very useful in such work: I had long before come to the conclusion that silkworm gut was the best suture, and the difficulty with the other needles had been their size, resulting in such a large stitch hole that subsequent infection was almost unavoidable. I at first used a pair of ordinary straight aspirator needles of small size. Through one I threaded a silkworm gut double, through another I placed a single strand. After paring the edges of the cleft I passed the needle loaded with the double suture and pushed the loop through; afterward, opposite to this, I passed the needle with the single thread. It was very easy to engage the single suture in the loop and withdraw both needles, the loop pulling the single suture through, leaving it in the proper position to be tied. One needle was then threaded with another single thread and the above procedure repeated until sufficient sutures were in place.

When all were tied and the operation was complete, the

resulting stitch holes were small and could be placed exactly where you wished. Fig. 1 illustrates, better than I can describe, the *modus operandi* of this little trick. It has been a very satisfactory method with me and I hope some of the fellows will try it.

The second of the two tricks is the use of a very simple little instrument for removing a silk ligature from a pedicle wherever it may be situated. I have used it only on the pedicle after nephrectomy, but can readily see how it could be used elsewhere—in a large-size pedicle of an ovarian cyst where co-existing infections were present or might be

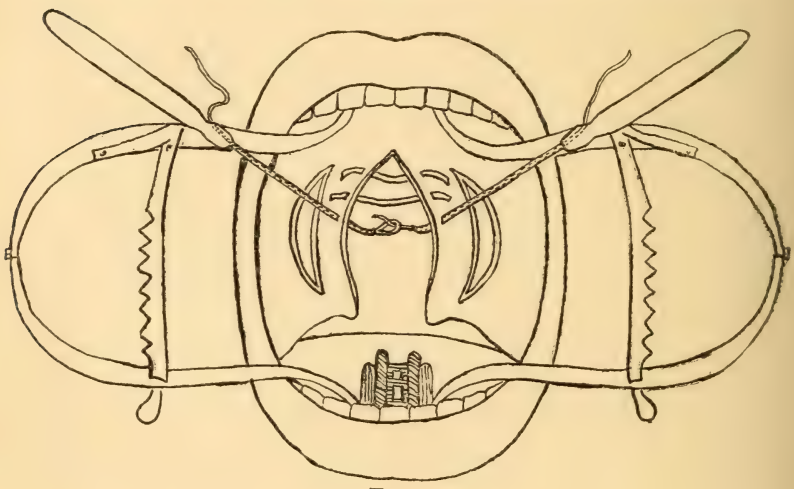

FIG. 1

expected, or in a vaginal hysterectomy where it was deemed best to use silk ligatures. Some ten years ago I suggested a more elaborate contrivance for this purpose, but failed to get it published, though it was reported and demonstrated. This was the use of a very fine platium wire left under the ligature at the time of tying, and brought to the surface by being attached to a heavier piece of copper wire covered by a rubber drainage tube; when the time came to remove the ligature an electric current was applied, which instantly heated the platinum wire which burnt through the ligature.

About a year afterward, Dr. Cleveland, of New York, thought of and demonstrated the same thing in New York and I believe put it to practical use.

Some five years ago I found myself anxious to get rid of a silk ligature on a kidney pedicle which I feared would be a long time in coming away spontaneously, and went to the instrument case to pick out a long slender pair of scissors to make the attempt to remove it, when I noticed an old silver catheter of small size. It immediately occurred to

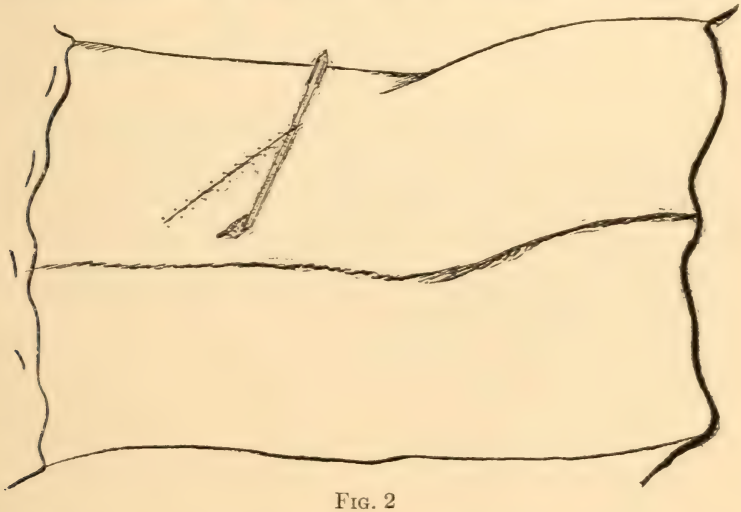

FIG. 2

me that I could use it, and this simple little instrument is the result which has served me a good turn a number of times.

I may be behind the times in advocating the use of silk as a ligature material. Some of you may say, "Use catgut and you will not need your instrument." Well, out in my country a fellow sleeps better if he knows there is a silk ligature on his case of nephrectomy, and as these cases, as a rule, are cases for drainage, and this little contrivance can remove a ligature at any time, without inconvenience to the patient, I submit it for your inspection and consideration.

Fig. 2 shows how simple it is and how easily it does the

work. The ligature or ligatures are brought out at the lower end of the wound and confined there by a plaque of adhesive plaster. At the end of ten days, a silkworm gut is tied onto the outer end and is used as a guide to thread the silk through this instrument. The distal end of the instrument is cut in such a way that one side is longer than the other, and this is made sharp. When the ligature is made taut, the knot is drawn up into this tube, then a turn or two cuts it off on one side without injuring anything adjacent.

## DISCUSSION.

Dr. E. Denegre Martin, of New Orleans.—I have never used the silver tube or cannula in such operations as reported by the doctor, but have used it for a long time in threading sutures for wiring bone and have found it of great assistance. Those who have had experience know how difficult it is to pass a suture through the newly drilled hole in the end of a bone, especially when the soft tissues interpose and cover the opening.

# ON THE TREATMENT OF HEMORRHAGE BY DIRECT TRANSFUSION OF SIMILAR BLOOD.

By George Crile, M.D.,
*Cleveland.*

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In this paper I shall present first a summary of the data on which transfusion is based, then report a typical case of transfusion for acute normal hemorrhage, one for pathological hemorrhage, and one for secondary anemia.

The transferrence of whole or modified blood by various methods for numerous purposes from one individual to another of the same or of an alien species, has been practised in many parts of the world for at least four centuries.

A critical historical review of this work with reference to the results accomplished may be summarized as follows:

The greater part of it was done before the development of chemistry, physiology, pathology, and bacteriology, *i. e.*, before the establishment of a scientific basis of medicine, before the establishment of good hospitals, before the period of good surgical instruments. It was, therefore, done crudely and empyrically. There were many accidents arising from infection, from clotting, from the use of alien blood, and from unfortunate selection of cases, so that with the advent of normal saline solution as a substitute for blood, transfusion of blood was no longer practised.

The work of Carrel, his predecessors and associates, has given us a method of bloodvessel anastomosis which has overcome the greatest practical difficulty in reopening this field of investigation. Is it not opportune, therefore,

with the twentieth century status of medicine, its more exact knowledge and increased resourcefulness, whereby most of the dangers and many of the limitations of the past may be overcome, to put the whole matter to a scientific test and endeavor to establish limitations as well as values?

The basis for the clinical use of transfusion of blood in the treatment of hemorrhage is as follows: that normal blood of an individual of one species is physiologically equivalent to that of any other individual of the same species and is, therefore, interchangeable; that the vascular systems of two individuals may be united by an end-to-end anastomosis of vessels; that such union may be accomplished so as to approximate intima to intima; that the blood transferred across this artificial continuity does not come in contact with any foreign body or any tissue but the intima; that under these conditions blood will not clot and the rate of flow and the amount transferred are under immediate control. The technique, however, is not easy and must be carried out with the greatest precision.

### *Illustrative Cases.*

A typical case was one of hemorrhage from the bowels. The patient was a woman, aged fifty-four years. She belonged to a sturdy race; had been accustomed to hard work and her family history was negative, there being no record of malignant disease or tuberculosis. At the age of thirty she had smallpox and the usual diseases of childhood. Menstrual periods began at sixteen. She had seven children, the youngest thirteen years of age. About three months before the operation there was a severe hemorrhage at stool. There was a discharge of a large amount of blood at the time of the stool and also several discharges of blood later in the afternoon. She had been constipated and with each bowel movement, from that time on and at other times, there

would be hemorrhage. Many times there would be desire to go to stool, when a small amount of blood would be passed. She became greatly reduced although she was under treatment from the time of the first hemorrhage until the time of the transfusion. Many times she would faint. A month later she had a severe vomiting spell lasting fourteen days, and she has had nausea and vomiting many times since. She had marked backache, no abdominal pain, no fever. She had but little appetite and lost steadily in weight. Every remedy taken, internally, for the control of the hemorrhage and taken by injection and suppository was tried without avail and she finally became bedridden.

At the time of the transfusion the patient was greatly emaciated; her complexion was a deep lemon yellow and no color of circulating blood could be observed in her face, fingers, or in any superficial part. There was edema of the extremities. On physical examination there could be found no tumor, hemorrhoids, fissure, or fistula. A marked anemic murmur could be heard over the heart. The pulse was 112, low tension, dicrotic, and the temperature slightly subnormal. Blood count showed 1,200,000 reds; hemoglobin, 12 per cent. She was obviously not a surgical risk, and transfusion was proposed as a temporary measure, hoping to improve her sufficiently to have her endure, with safety, anesthesia for exploration, and operation if necessary.

The patient's son served as donor. The transfusion was accomplished without incident. The red count rose from 1,200,000 to 2,450,000; the hemoglobin, from 12 per cent. to 40 per cent. The patient felt invigorated and buoyed up, she smiled, her face had become full, the orbital spaces and lines of expression becoming markedly diminished. All her tissue was fuller. The superficial vessels were distinctly noted. The pink in her face, hands, nails, and ears was distinctly marked. The next meal she showed a keen appetite. Her feeling of well-being continued and improvement was noted every day. From the day of the transfusion no blood

has appeared up to the present time—a period of three months. She gained rapidly in weight and strength and soon was up and about the hospital, and in every way her condition rapidly improved. Upon returning home she soon took up her household duties and at the present time is in normal health.

The first case of post-operative hemorrhage that seemed favorable to transfusion was one of intractable hemorrhage following nephrotomy for renal calculi. The usual means of treatment had been tried without avail and the case seemed hopeless. The patient was a man twenty-three years of age in whose family there had been no tuberculosis, malignancy, or kidney disease. His health had always been good, his appetite was good, and the bowels regular. At the time of the examination he felt fairly well with the exception of occasional attacks of pain in his left side. There was pus in the urine and occasional red blood cells were found. The *x*-ray showed a distinct shadow from the left kidney, which, with the other symptoms, completed the diagnosis of renal calculus.

By an oblique lumbar incision the kidney was exposed, lying well up underneath the costal arch and quite firmly adherent to the peritoneum. It was enlarged, especially in its long axis, and extremely vascular. It was at once obvious that it would be almost impossible to deliver the kidney through the wound without dividing the twelfth rib, which was then done. After considerable difficulty the kidney was displaced outward sufficiently to give us control of its blood supply and provide room for the incision. The capsule of the kidney was normal in appearance excepting an increased vascularity. The parenchyma was rather soft and unusually vascular. A mid-pole incision 4 cm. long in the line of the long axis was made. A large stone was found occupying the entire pelvis and all the calices. This was carefully removed in several pieces, sparing the kidney as much trauma as possible. The granulation tissue in which

the stone was embedded bled very freely on extracting it. Pressure upon the renal artery controlled the hemorrhage promptly. After a careful exploration of all the calices and the upper ureter, the wound was snugly packed with gauze which controlled the hemorrhage. Inasmuch as the calculus pocket was infected, the wound was closed only part way by sutures, the remainder being packed. The patient left the table in good condition although there was a considerable amount of blood lost.

In the first twenty-four hours there was only a slight amount of oozing from the dressings, but there was rather an unusual amount of discoloration of the urine. The second day his condition seemed favorable though there was still some oozing. On the third day the hematuria was markedly increased, likewise on the fourth, in spite of all the measures that were adopted to oppose it.

On the morning of the fifth the situation had grown more critical and the packing was removed in order to make a fresh and perhaps more effective hemostasis by a new packing. While doing this there was a very profuse hemorrhage, apparently a tissue and granulation hemorrhage, not from a larger vessel. In this instance the hemorrhage was almost equally profuse through the dressings and the ureter. The bladder was entirely filled with blood, which was voided as a full stream of almost pure blood directly after the dressing. The patient's condition had become very critical and considerable anxiety as to the outcome was entertained. During the day he was under active treatment for combating the effect of the hemorrhage as well as to prevent further hemorrhage. He lost steadily during the day and the oozing continued until the latter part of the afternoon, when it became obvious that the end was near unless some radical means of relief could be obtained. Up to this time he had the benefit of posture, of bandaging up to the costal borders of the lower extremities and abdomen, of repeated saline infusions, of stimulants, and of heat.

In spite of these measures he was steadily growing worse, and at four o'clock his blood count was 1,800,000; hemoglobin, 24; blood pressure, 60; pulse scarcely perceptible, at the wrist ranging between 155 and 170; respirations were shallow and rapid, the extremities cold, and he was well into the terminal stage of hemorrhage.

The following dilemma presented itself: the patient was rapidly losing ground and the oozing continued in the face of every resource at command. It was obvious that he would not endure anesthesia, and the position of the kidney was so deep and far beneath the ribs that it would not be possible to reach the vessels without anesthesia. There seemed no chance for the patient's recovery either by the line followed or by resorting to further surgical measures. In view of the experimental research, a preliminary report of which was published in the June number of the *Jour. Amer. Med. Assoc.*, it seemed justifiable to resort to transfusion on the ground that, could the patient be given a sufficient amount of blood and be sufficiently revived so that he could endure ether and nephrectomy, the day might be saved.

A brother of the patient offered his services as donor. Both were given $\frac{1}{4}$ grain of morphine and each placed upon an operating table with their heads in opposite directions. Under cocaine anesthesia the radial artery of the donor was dissected up 8 cm., the distal end ligated and divided close after the proximal part of the artery was secured by means of one of the author's screw clamps for closing vessels. The adventitia was then drawn well over the end and cut off snugly, a moist sponge protecting the exposed part while the median basilic vein of the donee was likewise prepared. The right arm of both donor and donee was then placed in close apposition to each other, resting on a little table between them. It was not difficult to bring the divided ends of the radial artery in apposition with the divided end of the median basilic vein of the donee. A No. 16 needle armed with one film of No.1 Chinese silk anointed with sterile vaseline

was then introduced through the wall of the artery and of the vein, tied, and cut 8 cm. long. Another such guy suture was placed at two other points in the circumference of each vessel so as to divide both vessels into three equal parts. The latter ligatures were likewise cut 8 cm. long. There were then three guy stitches placed through the wall of the artery and of the vein and tied. This brought the divided ends of each vessel in close apposition to the other and simultaneous traction on each of the three guy sutures converted the circular openings into triangular ones. By means of a similar needle and suture anointed with vaseline, a continuous row of through-and-through stitches placed close together, the needle passing through all the coats of each vessel, made a continuous suture and approximation of the artery and vein, forming a perfect anastomosis. On removing the clamp it was found that not a single drop of blood escaped from the suture line. Neither donor or donee were told when the transfusion would be made, and, indeed, it was made without their full knowledge.

Blood-pressure observations, pulse, respiration, blood counts, and hemoglobin estimations of both donor and donee were made prior to the beginning of the transfusion. Like observations were made just after, later in the evening, and daily following that.

On opening the screw clamp upon the artery of the donor, the red, pulsating wave passed at once through the vein, which dilated and was perhaps four times the size of the artery. The stream of blood passed across very satisfactorily and the results were closely noted, observations being made simultaneously upon both donor and donee. The blood pressure of the donee gradually rose from 60 to 94; the pulse fell from 160 to 120; the respirations gradually changed from a shallow panting to deeper, slower rhythm, from 48 to 28; red blood cells rose from 1,800,000 to 2,600,000; the hemaglobin, from 23 to 42. The most striking change, however, was noted in the face of the donee. The cold,

clammy, ghastly, pale-yellow complexion gradually diminished, the orbital spaces gradually filled, and finally a tinge of red, then a diffuse red appeared on the lips. The transformation was precisely similar to that of one coming out of a faint. At the end of thirty minutes this result was obtained and his condition was so improved that it then seemed safe to stop the transfusion entirely and do a nephrectomy.

The donor was then detached, the vessels tied, the wounds sutured, and both arms dressed. In the meantime ether anesthesia was carefully administered to the donee and the wound reopened. In removing the packing there was again a gush of blood. The swelling of the kidney and edema of the soft parts made it necessary to increase the incision. After very considerable difficulty the kidney was dislodged, the pedicle securely tied, and the kidney cut away. He was at once put into a rubber pneumatic suit and the blood pressure sustained for several hours after which, his condition being satisfactory, heavy layers of coarse cotton were placed over the extremities and lower abdomen. Three or four layers of heavy cotton-flannel bandages were then snugly applied. The bed was elevated and saline per rectum was given. The patient rallied well from the operation and passed through the night safely, but the next morning the bladder was found overdistended with urine and a large amount of clot from the hematuria of the day before. In the evening, after the operation, he was catheterized and four ounces of urine removed.

Under cocaine anesthesia and during continuous saline subcutaneous infusion, in the head-down posture on the operating table, the bladder was quickly opened and a large amount of ammoniacal urine mixed with quantities of infected blood clot was evacuated. He was then once more rebandaged from toes to costal borders and kept in the head-down posture under perfect rest. Hot applications were applied over the remaining kidney. Acute suppression followed. About twelve hours later uremia was quite

marked. Vigorous counterirritation and the use of acetate of potassium and digitalin were followed by renewed activity of the remaining kidney which, in the next twenty-four hours, secreted thirty-six ounces.

During the following two weeks there was no hemorrhage, the wound in the loin was more than half healed and the bladder wound fairly well under way of healing, though a severe cystitis persisted. Suddenly, and without warning, a very considerable hemorrhage occurred from the bladder. This hemorrhage continued for forty-eight hours and showed no tendency to cessation. Large quantities of adrenalin were injected into the bladder, ice bags applied, and all the remedies known were used, but the oozing was not modified.

On the fortieth hour after the hemorrhage began, the hemoglobin having fallen to 23, the red count to 1,800,000, and the oozing continuing, an attempt was made to stop the oozing by administering fresh blood from another brother. This was done under cocaine anesthesia. After each had been given a hypodermic of morphine with atropine, the radial artery of the donor was isolated, but found to be very small and to have a number of branches. These branches were all carefully separated and tied with a film of silk. The vessel was then secured by means of small special clamps, the distal-end ligated, and the vessel divided squarely with sharp scissors. The adventitia was pulled out over the end of the lumen and cut off. In a similar manner a small vein on the internal aspect of the forearm of the donee was prepared. Both arms were then placed on a table and the anastomosis made. The union was so perfect that not a drop of blood escaped as the transfusion began. The donee complained of precordial distress and protested loudly that he felt sick. His breathing was markedly increased. Later, he was seized with a chill accompanied by considerable circulatory disturbance. The transfusion had been going on at intervals for about one-half hour. The pulsation

could be distinctly felt up the veins of the forearm up to the axillary space. His volume of blood now consisted of a mixture from three individuals. The immediate improvement though notable was not so marked as in the previous transfusion. After he was returned to his bed he regained his composure and a marked transformation was observed. All the symptoms were greatly improved, the blood pressure, the hemoglobin, and the red count.

Although no other treatment whatever was administered the bleeding ceased after several hours never to re-appear. The wounds all improved immediately, the granulation tissue became red and active, and rapid healing followed. At present his red count is 6,500,000; hemoglobin, 100; he is in good health and is engaged in his former occupation.

The third case I shall cite was one of secondary anemia following cancer of the gall-bladder with obstruction of the ducts, chronic jaundice, and subcutaneous, nasal, uterine, and rectal hemorrhages. The patient was a woman aged forty-two years, whose family history showed but one instance of carcinoma; they had been long lived, two grandparents having lived to be over ninety years of age. The patient herself had been well until four years previously when she had a miscarriage. During the year previous to the operation her health became gradually impaired and at the time of the operation she was jaundiced; there was no temperature, no pain, the stool was gray, urine highly colored, and there was apparently complete obstruction to the flow of bile. Eight days before the operation there was acute epigastric pain and two days later two small stones were passed. In the meantime the normal period for menstruation fell due and the flow continued for twelve days, at which time she was curetted; following this the flow stopped. At this time blood in the stool was noted, which continued at every stool for two and one-half weeks.

On examination, a hard, slightly tender mass had been palpated over the gall-bladder. There was severe petechial

hemorrhage and diffuse subcutaneous discolorations, rectal bleeding was continuous, the pulse and temperature elevated, and the jaundice deepened. The patient was sinking rapidly. The point of making the transfusion was to give fresh coagulable blood which might arrest the hemorrhage. The patient's brother served as donor—a strong healthy man, aged twenty-eight years, and weighing 180 pounds. Both donor and donee were given $\frac{1}{4}$ grain of morphine twenty minutes prior to the transfusion. An anastomosis by the technique heretofore described was made between the proximal end of the radial artery and the proximal end of a superficial vein near the elbow. Neither donor or donee felt any pain or discomfort during the dissection or the transfusion. The radial artery was unusually large and the vein selected was presumably only a little larger, but because of its empty state it was misjudged and proved to be about twice as large. However, there was no trouble experienced in making a blood-tight suture line despite the discrepancy in size. A continuous flow of blood was maintained for forty minutes, when it was noted that the donor showed slight evidence of fainting. The transfusion was then terminated.

The donee manifested the following phenomena: The pulse tension steadily increased, the rate fell from 110 to 80; the face gradually filled out; lips and ears became pink, though on account of the deep jaundice the effect upon the superficial circulation of the skin could not be clearly seen. However, her condition rapidly improved and at the end of the transfusion instead of presenting the appearance of one in collapse she felt stronger and refreshed.

During the transfusion the bleeding from the nose, which was progressive, ceased. Immediately following the transfusion the bleeding from the uterus also ended. She passed a comfortable night and the following morning was in such good condition that it was decided to perform an exploration under cocaine and morphine anesthesia to accurately determine the cause of the jaundice.

A carcinoma of the gall-bladder with heavy metastasis about the liver and pylorus was found. It was clearly inoperable, not even the ducts could be isolated and drained. The wound was closed and no unusual amount of hemorrhage resulted. The operation produced but little effect upon the patient. In fact, she was told prior to the operation that a particular examination and, perhaps, some treatment by needle puncture, etc., would be made. Under these circumstances she did not know that the abdomen had been opened. The psychical factor was thereby eliminated and there was no appreciable change of any kind due to the operation. The three days following the operation there was temperature ranging from 100° to 102°. The bowel movements on the two days following were quite as black as before, and on the third day there was a considerable amount of clot passed from the bowel.

The first week following the operation and transfusion the patient continued to be greatly improved, looking better, more cheerful, and hopeful. Her appetite increased, the nausea and vomiting disappeared, and on the fourth day after the operation the stool for the first time in three weeks was without blood. Following the transfusion there was a disappearance of all the subcutaneous hemorrhage. There was no bleeding from the nose, the uterus, or from the wound. The wound healed in due time and the patient gained during the first two weeks.

Thirty-five days have elapsed since the transfusion and operation and there has been no recurrence of hemorrhage of any kind. The patient's condition remains about the same; she has not lost in weight; her appetite continues fairly good; there is now bile in the stool; the urine continues heavily loaded with bile; the jaundice remains approximately the same.

In this case the pathological hemorrhage was pronounced and had an operation been performed without transfusion the patient on the one hand would not have been able to

endure it, and on the other hand, she would have undoubtedly bled to death. The cessation of the hemorrhage from all the observable points during the transfusion would lead one to conclude that it was due to the addition of fresh blood, which overcame the pathological condition of the patient's blood and, therefore, controlled as well as prevented hemorrhage. The patient was remarkably revived by the addition of the new blood and so far as one could judge her expectancy of life has been by that means distinctly increased. It would seem to be a case in which an especially heavy stress upon the method was laid because of the failure to relieve jaundice by the operation, also, because the hemorrhage was due to the jaundice of carcinomatous obstruction, which is known to be the most fatal.

Another feature of this case is of importance. The work of Gaylord and Clowes, of Ehrlich, Bashford, Loeb, Ewing and Beebe, and others, has shown that certain animals have an acquired immunity to cancer. In a measure this might be considered an observation on the effect of the direct transfusion of blood upon carcinoma. To be sure we do not know whether any natural immunity exists in the human subject, and, if so, whether the donor in this instance possessed such immunity. We, of course, do not know how many human subjects may have had a malignant tumor and may have recovered and thereby acquired an immunity. We have no direct evidence in this case that the malignant tumor was in any way modified. It is true that the patient, so far as can be judged, would not otherwise have lived five weeks longer. In fact, it is likely she would have lived but a few hours, as she was in collapse at the time of the transfusion. Yet, the addition of so large a quantity of fresh blood from a healthy individual into her circulation is a factor which we are, at the present time, unable to determine with accuracy sufficient to make deductions as to any influence the transfusion might have in sustaining the patient's health against malignant growth.

This is but a single case and is regarded with corresponding value, but on *a priori* grounds and on the ground of the very definite results following in the correct sequence, we are encouraged to hope that this method will prove effective in the very considerable class of pathological hemorrhages associated with jaundice.

Only the gravest and apparently hopeless cases have thus far been transfused. Altogether fourteen clinical cases have been observed. All were done painlessly under morphine and cocaine anesthesia. No unfavorable symptoms of importance have been noted. The donors have regained their lost blood in from five to six days. Every donee has felt immediate improvement. Striking evidence of this has been shown in the facies, the improved circulation, and the feeling of well-being and even buoyancy.

The technique is by no means simple and must be carried out with absolute precision. In no instance was there observed nephritis, hemoglobinuria, agglutination, hemolysis, or laking of blood. The transfused blood apparently exercises no unfavorable influence on its new host, and the latter none upon the new blood. The blood of individuals of the same species, therefore, seems to be physiologically interchangeable. Certain cases of pathological hemorrhage have been cured by the addition of fresh normal coagulable blood. Transfusion in a case of pernicious anemia was tried with totally negative results. Partly on the basis of Wright's work on opsonins and partly on other grounds two cases of transfusion, after bleeding in tuberculosis, have been done. In every instance in the research and the clinic the effect of hemorrhage has been overcome by the replacement of the lost blood by means of transfusion. This paper is but the report of progress, for we are not yet ready to reach conclusions or to make deductions or predictions.

## DISCUSSION.

Dr. J. M. T. Finney, of Baltimore, was asked to open the discussion. He said: The Society is to be congratulated on being the medium through which this most valuable piece of work is to be transmitted to the profession. I do not recall having recently heard a paper that has interested me more than this. I only wish we could have the opportunity of seeing Dr. Crile do some of this experimental work, and I would suggest, if possible, that arrangements be made for him to give a demonstration of his method on a dog before the members of the Society. I am sure they would be interested in seeing such a demonstration. Dr. Cushing, I feel sure, would be willing to help in making the necessary arrangements.

I wish to ask Dr. Crile a question for my own information. I did not quite understand why it is that a canula should not be used in making the anastomosis.

Dr. Samuel J. Mixter, of Boston.—Dr. Crile is very modest in claiming success for the method he has described. I think the members of the Association would like to hear him answer a question I asked him when he finished reading his paper, namely: Why this method could not be used very generally as a prophylactic measure? We all know the terrible, uncontrollable hemorrhages that occur after operations during deep jaundice, and which at present, in spite of giving various agents, have proved so intractable. Now, it seems to me, here is a great field for such an operation as Dr. Crile has described—that is, the giving of fresh blood to these deeply-jaundiced patients—bleeding, if necessary, from one person to give fresh blood to another. I should like to ask him, in the name of the Association, if he does not think in cases like this it would be of the greatest possible value. It is worth the while of all of us, who have to deal with these cases, to think whether it would not be well to try this procedure before operation, and not after the hemorrhage has taken place.

Dr. Robert T. Morris, of New York.—I would like to ask Dr. Crile how he explains the absence of hemolysis, and if he does not anticipate and fear hemolysis in a certain proportion of cases? This is one of the things that would appeal to me; yet, if I understood Dr. Crile correctly, he distinctly stated that it does not occur in these cases. In our grafting work, in transferring tissues from one animal to another, there is a well-known principle of intolerance, and this must apply with extreme force in a case

where blood carried from one individual is to be taken by the blood of another individual.

Dr. William H. Welch, of Baltimore, was asked to take part in the discussion. He said: I can only express my delight in having heard this admirable and interesting paper. It opens up a most interesting field of work.

Reference was made to the historical side of the work, and it has been pointed out that in the seventeenth century it was supposed that one of the greatest discoveries in practical medicine had been made in the transfusion of blood. Dr. Crile is doubtless familiar with the work done at Oxford and in London by that extraordinary little group of men—Bower, Willis, and Boyle—which subsequently laid the foundation for the Royal Society, and was one of the most interesting historical chapters in medicine. Wren, the great architect, was likewise engaged in his work at that time.

It is quite clear, that with all these special precautions, the behavior of the hemolysis of the blood is very different from alien blood, and that these methods permit of results that could not possibly have been obtained in the old days. The paper furnishes food for thought with regard to introducing defensive substances into an individual who has lost them; and it is difficult to predict what the outcome will be. We have to let out blood before we introduce new blood.

I would like to know why the blood could not come in contact with any foreign material, and whether the observations made have been based on a sufficient number of experiments to determine if a glass tube cannot be used through which to allow the blood to flow so as to simplify the technique.

As regards the control of hemorrhage from the kidney substance, in these cases, I would like to inquire whether Dr. Crile has had any experience with the administration of kerosene, and whether it is useful or not in these cases?

Dr. Crile (closing).—In reply to Dr. Finney's question as to whether or not a canula might be used, I will say that we tried various kinds of canula (glass and silver, as well as other varieties) and the number of times we obtained clotting was too great to hazard the use of it in practical work. We still hope that some such mechanical means may be devised. We have used hirudin and have found that if we inject it into an animal the blood will not clot; but, we found that edema of the lungs sometimes followed.

In answer to Dr. Mixter's question as to whether transfusion might be used in cases of profuse hemorrhage following operation, I see no reason why it should not be tried, although, I should not like to predict the result. The subject should be investigated.

Replying to Dr. Morris, hemolysis does not occur in using homologous blood.

# SURGICAL TREATMENT OF TUBERCULOSIS OF THE KIDNEY.

By Howard A. Kelly, M.D.
*Baltimore.*

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I PROPOSE to lay stress upon the incalculable value of radical surgery in the treatment of tuberculosis of the kidney, and to demonstrate that the recognition of this fact by the profession at large will greatly improve our statistics of renal tuberculosis. I feel strongly tempted, by way of introduction, to make an excursion into the literature of this interesting subject to show how surgery first gained its foothold in this new field, and how it has advanced by slow steps since then; and this inclination is the stronger, because the progressive movement has taken place almost entirely since I treated the first patient of the series upon which most of my opinions are based. However, as time is short, I proceed at once to the question of surgical treatment.

My experience with tuberculosis of the kidney, excluding patients seen in consultation with other surgeons merely for diagnosis, and several patients still under observation, covers 57 nephrectomies, 4 nephrotomies, and 1 partial nephrectomy.

These operations have been performed either in the gynecological department of the Johns Hopkins Hospital or in my private hospital. The first patient of this series came into my hands in 1889, and the last is now convalescing.

Before we discuss the question of operation, let us briefly consider the value of an expectant or medicinal treatment, by noting the course of the disease, in cases handled by this method, under careful observation. If we first clear the field

in this way, we shall be better able to gauge the significance of surgical treatment.

Personally, I have yet to see a spontaneous, natural, or medical cure of the disease where the tuberculosis has been clearly demonstrated, though I have seen many futile efforts in this direction. Some of my earlier patients underwent the general fresh-air and forced-feeding treatment, so useful in pulmonary phthisis, and I had the opportunity of watching them month by month as they diligently pursued the measures advised by my medical colleagues; but, not only did I never see a regression of the disease, but in nearly every case there has been a demonstrable advance. Added to this, a study of the specimens obtained by my nephrectomies does not afford a single instance of any kind of a healing process which preserves functional capacity in the affected part of the kidney. In cases where the kidney has been completely destroyed, and its ureter filled with caseous material or occluded, of which I have had a number, there has always been extensive bladder tuberculosis, and, in some instances, tuberculosis of the second kidney as well. It is true a sequestration or encapsulation of the diseased kidney associated with an occlusion of the ureter may lead to a pseudocure, and many such cases are recorded. Experience shows, however, that even in these instances the disease may long remain dormant, ready at any time to light up afresh.

In questioning nature's powers to heal unaided, we must also take into consideration the occasional extreme chronicity of the disease; the average duration of symptoms in my own group of 62 patients, before consulting me, was as much as three and one-half years. In 1 the symptoms had been present for thirteen years and there was some secreting tissue left in the diseased kidney, while its fellow was healthy; this is an extreme example. Again, *per contra*, a single year may see the onset of the disease in one kidney, its utter destruction, and the involvement of the bladder and the

second kidney. With the rare exception of the sequestrated kidneys, sooner or later the disease involves the bladder. Of my 62 cases, 40 had more or less extensive bladder tuberculosis. In some instances the bladder involvement takes place very early, while in others it is delayed for several years. I need not dwell upon the sufferings caused by bladder tuberculosis, when life is worse than the death, which comes with tardy feet through the involvement of the second kidney or metastases in other parts of the body. Without elaborating my argument I would express myself as to the origin and course of the disease as follows:

1. *Tuberculosis of the Kidney, not including General Miliary Tuberculosis, is always Unilateral at the Start.* Even autopsy statistics show that 50.5 per cent. of cases are unilateral, while studies of the separated urines and the postoperative sequelæ show that 54 out of my 64 cases were unilateral, a percentage of 84.4 per cent. Albarran found that 58 out of 64, or a little over 91 per cent., of his cases were unilateral; he has, therefore, been fortunate in securing his patients at an earlier stage. I consider my own group of cases as an advanced one, since the average duration of symptoms had been, as I have said, three and one-half years when I first saw them, and nearly two-thirds showed bladder involvement.

2. *Tuberculosis of the Kidney is Practically Always Hematogenous in its Origin.* There is no association between genital and renal tuberculosis by way of the genitourinary excretory channels or by the bladder. They may coexist independently or the infection may be transmitted by the blood.

3. *Primary Tuberculosis of the Bladder is Rare.* In a large experience with bladder conditions in women, I have only seen 1 case unassociated with kidney tuberculosis.

4. While a bladder tuberculosis cannot heal as long as there is a tubercular kidney above, it does respond to suitable treatment after a nephrectomy.

The operative procedures to be considered in this connection fall into several groups: nephrotomy, partial nephrectomy, nephrectomy including nephroureterectomy, and nephroureterocystectomy. Let us glance first at the results of nephrotomy and consider the indications for it.

1. *Nephrotomy* or *nephrostomy* is indicated in desperate cases of pyonephrosis, whether from a purely tubercular or from a tubercular mixed infection. When the other kidney is not tubercular, nephrotomy must always be followed by nephrectomy. When the remaining kidney is gravely involved in the tuberculosis, nephrotomy should be the only operative procedure, for in such cases it often effects a remarkable temporary improvement in the patient's general condition. In 4 cases in which I have used it I have seen great immediate relief of the symptoms, although the patients have all died within a year of the operation. As a preliminary to nephrectomy, nephrotomy may be of inestimable value. I have done it in this way seven times, and all of these patients are well today. Some of the seven were among the most unpromising-looking patients I have ever seen: bedridden, emaciated to the last degree, and running a hectic fever with a huge pus kidney, death seemed inevitable. Two months is usually a long enough period for such a patient to rally enough to stand a nephrectomy, which had then best be done by the intracapsular method.

2. *Partial nephrectomy* for tuberculosis has, up to the present time, had but a small range of application. Israel reports two unsuccessful attempts which he followed by nephrectomies; Fenger tried it faithfully and his patients all died; Bazy and Pousson have had similar experiences. Henry Morris reports a case in which he removed a tubercular kidney and then, at a later date, had to take out one-third of the other kidney for tuberculosis at one pole. His patient was well when heard from several years after the operation.

A study of the specimens from my cases show that in 5 the disease was localized and limited to but one pole. The

most interesting of these is a kidney which I show here: it has a double pelvis, two ureters, and a double blood supply.

The upper half of these two fused kidneys is destroyed by tuberculosis, while there is no disease at all in the lower half. Albarran has also reported a case similar to this. Such a case ought to have been treated by a resection as it affords the most favorable conditions imaginable for it.

I do not wish to advocate that hazardous operation, a partial nephrectomy, as a substitute for nephrectomy, but I do believe that further attempts should be made in this direction, in conditions which positively forbid a nephrectomy, as, for example, where both kidneys are tubercular and yet both are doing good work. The case in which I employed it did not prove a success as far as cure was concerned as she died eight months later. Her life was not shortened by the operation. Such patients are now left to a distressing fate, as a rule, a rapid *exitus letalis*. Attempts in this direction can do no harm, and there is a chance now and then of rescuing the better kidney. Let the rule be to take out the bad kidney first and then to resect the better one.

A rare condition, in which this operation is clearly indicated, is when one-half of a horseshoe kidney is tubercular. Lennander, of Upsala, has reported a successful case.

3. The operation of widest application, and that which should always be done if possible, is nephrectomy. In the comparatively exceptional case of one-sided kidney tuberculosis, with the other kidney healthy and no tuberculosis evident in the bladder or elsewhere in the body, no one, in view of the well-established facts, ought to dispute the imperative indication to remove the diseased kidney. Unfortunately, however, it is in this very group that dilatory measures are more often advised. It is true the patient often seems to be in robust health and the local symptoms appear trivial; nevertheless, it must not be forgotten that no case of spontaneous healing has been demonstrated, while the diseased kidney, so long as it is harbored, is a constant menace

both to the bladder, to the other kidney, and to life itself. My test for spontaneous healing calls for a preliminary accurate and positive diagnosis of tuberculosis of the kidney with the subsequent demonstration that the kidney has recovered, has continued to function, and has shown no evidence of tuberculosis over a long period after the healing is alleged to have occurred.

The operative treatment of the cases of unilateral tuberculosis of the kidney, without bladder involvement or with only slight involvement about the ureteral orifice, and without serious tuberculosis elsewhere, promises a great deal, as my cases teach. Excluding two patients operated too recently to be reported, I have had 20 cases of nephrectomy in which this favorable condition was present. All in my list, without exception, remained in perfect health over periods extending from two to ten years. *In view of these results I cannot too strongly urge that the proper time to do the nephrectomy is as soon as convenient after the diagnosis has been made.* When the disease is further advanced, the difficulties multiply rapidly, the likelihood of disease in the second kidney is increased, grave bladder troubles are present, rendering topical diagnosis very difficult or even impossible, perirenal changes occur greatly adding to the seriousness and dangers of the operation. Out of 36 of these advanced cases, I have had 4 primary deaths and 5 more within periods of from two months to four years. The bladder conditions have, in some cases, demanded treatment for months and months before healing was effected.

In considering the limits of nephrectomy it is convenient to take up the complications met with in my cases, and to consider them separately as regard their bearing on this question. I will take up in order, tuberculosis of the bladder, tuberculosis of the genitalia, tuberculosis elsewhere in the body, and tuberculosis of the second kidney, complicating tuberculosis of a kidney upon which nephrectomy is proposed. I will then consider certain affections of the second kidney

which are not tubercular, but whose existence complicate the situation.

1. Involvement of the bladder, far from being a contra-indication, is a marked indication for nephrectomy. As long as the tubercular kidney above pours down infectious material, the bladder cannot be healed; whereas, it often yields promptly to simple treatment after the nephrectomy.

My series includes 36 cases where extensive bladder tuberculosis was present. Out of this number there were 4 deaths immediately dependent upon the operation, a mortality of 11 per cent.; 3 more died within a year, 1 after two years, and 1 after four years, making a total of 9 deaths or 25 per cent. mortality. Eighteen patients, or 50 per cent. of the total number, have been entirely cured for periods of longer than two years and up to twelve. Seven patients, all of whom have been operated on within a year and a half, still have residual bladder trouble, 1 has had it for two years, and 1 patient for over four years. The last 2 patients have not taken regular treatment; 1 of them has been through a pregnancy, and both may be considered as having neglected themselves.

Excluding the cases where bladder trouble is still present the total mortality for this group of cases has been 33 per cent., unfavorable indeed compared to the simple cases with no mortality, but still excellent in view of the certain death of all without operation. It is of interest to consider the causes of the primary deaths. In 2 of the 4 cases it was from anuria; these cases were quite similar. In each, one kidney had been entirely destroyed and changed into a fibro-fatty mass, and in each, the other kidney was tubercular; both patients were in extremely bad general condition. The bladders were so small, contracted, and ulcerated, that a separate study of the urines of the two kidneys was found impossible. One patient had acquired, in the active kidney, a sudden pyemic infection which led to the nephrectomy.

The other had a pelvic-tubal tuberculosis in addition to

the double renal involvement. The abdomen was opened and the kidneys and ureters palpated; on the side which was entirely inactive the kidney and ureter were of normal size and the ureter of normal appearance, while on the other side the kidney was twice the normal size and the ureter thickened. The situation was desperate and I do not know how the ill-result could have been avoided. One of the remaining 2 cases of the 4 primary deaths died of peritonitis, and 1 of what seemed a general infection, but no autopsy was made.

Of the secondary deaths, 3 of the 5 were from involvement of the remaining kidney. One of them, known to have tuberculosis of the second kidney at the time of the operation, died within two months; the other 2, with clear urines of the second kidney, died within two and four years, respectively, of the operation. Of the 2 remaining deaths, 1 was from heart failure, occasioned by an operation to remove the tubercular ureter which had been left in place, and 1 from an unknown cause, forty days after her operation.

2. Tuberculosis of the genitalia is a more frequent complication in men than in women; my experience with this complication is limited to the latter, so far as this associated localization is concerned. I have had 5 cases in this category, of which 3 have entirely recovered, 1 has only recently been operated upon, and 1, as already cited, died from the operation. In these cases, the tubercular genital organs must always be removed, as well as the kidney. Rovsing has recently reported 10 cases of epididymal and renal tuberculosis which he has cured, showing that the results in men also are good in this class.

3. Tuberculosis elsewhere in the body, if not extremely active, is no contraindication to operation. I have had in all 18 cases of coexisting tuberculosis; pulmonary, joint, or glandular. After nephrectomy and under general treatment 13 have recovered and have been, apparently, entirely

well for over two years. Three have been operated upon within two years and three have died.

4. Tuberculosis of the second kidney. In cases where the worse kidney is not secreting at all, or very little, its removal seems to benefit the sounder kidney and to help the general condition remarkably, if only temporarily. This is true especially when the worse kidney is actively manifesting the disease. I have had 7 such cases, when the nephrotomies are included, and I must confess that it has been but a palliative measure in my hands. Except where the patients are rapidly going down from pyonephrosis, I think that the prolongation of life it affords can be measured in months, and the relief of symptoms is not great. As already stated, the relief of a pyonephrosis, even when the other kidney is tubercular, often leads to a remarkable temporary gain in general condition.

5. The presence of hyalin and granular casts and albumin in the urine from the second kidney does not contraindicate operation. In 4 cases out of 5 they disappeared shortly after the nephrectomy, and in the fifth case, where they persisted and demonstrated a true nephritis, the patient has been actively engaged in teaching school for seven years.

6. In three of my nephrectomies there has been a well-defined colon bacillus pyelitis of the non-tuberculous kidney. These patients are alive thirteen, seven, and four years, respectively, since their operations.

It is evident from these considerations that nephrectomy has a wide range of utility in the treatment of renal tuberculosis. I cannot here go into the operative and other treatments of tuberculosis of the bladder. They demand a consideration as wide as that given the condition in the kidney in this paper. In general, it is a fact that the cases in which the ureters are removed respond more quickly to bladder treatment than those in which they are left in, and the incisions close sooner. If the entire ureter is thickened

and tubercular, provided the patient's condition warrants it, a complete ureterectomy should also be performed; an operation which I have called nephroureterectomy. In most of my cases there was thickening and tenderness of the vesical end of the ureter, showing a tuberculosis at that level. This was found in case after case where the upper part of the ureter was normal. In most of these patients, after a nephrectomy, the vesical condition healed spontaneously. The ureters in tubercular cases, when not removed, should be divided with a Paquelin cautery and sunk. One of the most obstinate sinuses I have had was in a case where I sutured the ureter in the lower angle of the incision after Israel's recommendation.

In pyonephrosis, requiring a preliminary nephrotomy, the intracapsular method has given excellent results, and although the sinus may persist for several years, it will ultimately close. I have had 1 case to remain open for four years.

The statistics which I have presented to you clearly demonstrate that in any satisfactory consideration of tuberculosis of the kidney, it is necessary to classify and group the cases in order to obtain a just estimate of the operative results. I find that the literature of the subject is not of great assistance in securing these comparative statistics. In most of the statistical reports there is no more detailed consideration than the statement of the primary mortality. Taking the eases as they run, the primary mortality will depend upon the skill of the operator as well as upon the severity of the cases with which he has to deal. There can be no question but that given earlier cases and simpler conditions, the primary operative mortality will drop many per cent.

Grouping my cases all in one, I have had 57 nephrectomies with 4 deaths, giving a primary mortality of 7 per cent.

In 1900, Pousson collected 600 cases reported in the literature and found 21.33 per cent. mortality primary.

Albarran reports 57 cases and 2 deaths, 3.5 per cent.; Rovsing, 47 cases, 3 deaths, 6.3 per cent.; Israel, 70 cases, 12 deaths, 17.8 per cent.; Kuemmel, 42 cases, 6 deaths, 14.3 per cent.; Pousson, 25 cases, 3 deaths, or 13 per cent. (personal).

These statistics as to primary mortality are, of course, of great value; but if the class of cases operated upon is clearly defined, we have a better basis for estimating the dangers of the operation in a given case. They give us, also, but a slight hint as to the ultimate curative power of the operation. Kuemmel states that 32 out of 43 of his cases, or 74 per cent., were living some time after the operation. Albarran was able to follow 39 out of 53 of his cases, and stated that 5 of these 39 died of further development of tuberculosis. Pousson, in his collection of statistics from the literature, estimates that 50 per cent. are living after several years. No mention is made in any of these statistics as to whether the patients were really well, and this of course is an essential factor since it is possible for a patient to live several years with only one kidney, and that a tubercular one. My own collection is unique in this respect in that every patient has been followed up, not one having been lost sight of. From all who are now living, reports have been secured and careful urinary examinations have been made. Many have been seen recently, and cystoscopic examinations have been made, and only those cases in which the urine has been found normal, and where the patient has had no symptoms of disease, has a cure been registered. I think that in the future all reports of tubercular kidneys treated surgically ought to be precise in this respect.

In conclusion I would again emphasize the hopelessness of medicinal and hygienic measures alone, and emphatically condemn the waiting treatment of tuberculosis of the kidney.

I wish, therefore, to urge particularly a prompt, early operation. We have ceased to discuss the advisability of operating promptly in acute appendicitis, and yet here is a condition

more imperatively demanding operation, and that too an early operation; a condition even more hopeless in its outcome than any given case of acute appendicitis. When the profession at large comes to realize this fact, and call upon the surgeon to assume the early treatment of renal tuberculosis, then the results of surgical interference will grow better *pari passu*.

# TUBERCULOSIS OF THE KIDNEY.

By Charles P. Noble, M.D.,
*Philadelphia.*

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In no other part of the general subject of tuberculosis has greater progress been made in our knowledge than in tuberculosis of the kidney. As recently as ten years ago the teaching of recognized authorities concerning tuberculosis of the kidney was not only very incomplete, but, as subsequent investigations have shown, it was also largely erroneous as to the mode of invasion, the gravity of the lesion, and the nature of its treatment. I shall not attempt a systematic study of the subject, but wish to present my own experience, more especially with nephrectomy for tuberculosis, and to discuss certain problems which enter into the general question from the standpoint of that experience.

My first nephrectomy was performed in 1895, at which time the operation was relatively a novelty. At that time it was believed that tuberculosis of the kidney was frequently if not usually bilateral, and it was held that when it was not a part of a generalized tuberculosis it was usually a secondary condition due to tuberculosis of the bladder with ascending infection. For these reasons the field for curative treatment was believed to be a very small one, and the mortality of nephrectomy for tuberculosis was estimated as at least, 33 per cent.

The views current ten years ago were almost exclusively the result of the teachings of pathologists, based upon the findings at autopsy, and, therefore, were based upon the

conditions found after the disease had run a fatal course. It is easy to understand why pathologists formed erroneous views from the evidences presented at autopsies. When the kidney involvement was moderate, the disease was usually secondary to primary tuberculosis of the lungs; and when the kidney involvement was primary and extensive, the disease had run its course and the ureter and bladder were found to be involved. The pathologists inferred that the tuberculosis of the bladder was primary, and the infection of the ureter and kidney secondary, from the analogy of the infection of the ureter, pelvis of the kidney, and kidney, with other micro-organisms. As is well known, when not due to pyemia or to bacteriemia, ureteritis, pyelitis, and pyelonephritis are commonly due to ascending infection from a primary cystitis. No evidence has come under my observation that tuberculosis of the kidney in women is ever due to ascending infection. On the contrary, in 50 per cent. of the cases the bladder was but little if at all infected, and in all of the cases the ulceration of the bladder, or cystitis, has undergone either spontaneous recovery or has recovered after local treatment subsequent to the removal of the primary focus of the disease in the kidney by nephrectomy. Cystitis existing as a complication of tuberculosis of the kidney is usually due to infection from the pus entering the bladder through the ureter. In a considerable percentage of the cases, however, there is a mixed infection, and the cystitis is due, not to tuberculous infection, but to infection from some other organism.

My experience does not warrant me in expressing an opinion concerning the relative frequency of primary tuberculosis of the bladder in men. The fact that tuberculosis of the epididymis and of the vesicula seminalis is not rare would indicate a more substantial basis for the occurrence of primary tuberculosis of the bladder with ascending infection in men; but it is interesting to find that the more recent studies of this subject show that the older view concerning the frequency of primary tuberculosis of the bladder is

almost, if not quite, as erroneous with reference to men as it has proved to be with reference to women.

Diagnosis. The diagnosis of tuberculosis of the kidney is usually difficult, except in late cases in which the kidney is much enlarged by pus contained within its capsule, or in which the presence of a perinephritic abscess, together with the history of the patient, indicates the probable nature of the malady. In but a minority of the cases were the patients referred to me with a diagnosis of disease of the kidney; in the majority of them the diagnosis was cystitis. The first symptom, in most cases, is increased frequency in urination. This usually persists and becomes more marked. Examination of the urine shows the presence of pus, and the urine is usually acid. Frequent urination and persistent pyuria with acid urine is a combination of signs which should always raise the suspicion of tuberculosis of the kidney. The suspicion is increased if the patient shows evidence of failing nutrition and discomfort in the region of one or the other kidney. Blood in the urine is a frequent symptom, usually microscopic in amount, but occasionally sufficiently free to cause a definite hematuria. Pain in the kidney is rarely the first evidence of tuberculous involvement, and when present is usually one of the evidences of extensive involvement of the kidney. Intermittent pain, more or less colicky in character, is at times due to partial obstruction of the ureter with pus, and at other times to stricture of the ureter with accumulation of urine or pus above the stricture. Rarely, the presence of a mass in the region of the kidney, with or without local pain, is the first sign to draw attention to the kidney involvement.

The symptoms and signs already given are sufficient to raise the presumption of the presence of tuberculosis of the kidney, especially when the patient has a tuberculous family history; but a positive diagnosis, except in the most advanced cases, can only be made by establishing the tubercular character of the lesion. For a time it was thought

that this could be determined by finding tubercle bacilli in the urine. Considerable experience with the method has convinced me, in common with many others, that this method of diagnosis is fallacious and unreliable. A negative report may mean that the technique employed by the examiner was poor, or that his patience was soon exhausted; and a positive report brings with it a doubt as to whether the examiner has confounded the smegma bacillus with the tubercle bacillus. Until means of differentiating between these two organisms by bacterioscopic methods is discovered, the supposed finding of tubercle bacilli in the urine is by no means positive evidence of tuberculosis.

Since my fifth nephrectomy in 1899, I have relied upon the method of injecting the urine into guinea-pigs. One of the chief objections to this method is the time which must elapse between the injection and the diagnosis, usually five or six weeks. It has the great advantage, however, of determining the presence of tubercle bacilli, even in early cases of tuberculosis of the kidney. Like all laboratory methods, it is open to errors in technique. In the 5 cases in which a positive report has been received, the diagnosis was confirmed by subsequent operation and pathological examination; and of numerous cases in which a negative report was furnished, in none has the subsequent history of the patients shown an error. I am familiar, however, with 1 case in which a positive report was furnished through a mistake. In this case, the surgeon was not satisfied with the diagnosis and sent an additional specimen of urine, from which a negative report was received. The subsequent history of the patient proved that tuberculosis did not exist. In this case, either the guinea-pigs were mixed or a guinea-pig already infected with tuberculosis was injected, and thus an erroneous report was furnished. At the present time a positive report from the injection of urine obtained by ureteral catheterization is the most reliable evidence of tuberculosis of the kidney.

Cystoscopy furnishes evidence of value. The trained examiner is able to form a pretty accurate conclusion as to whether the extent of the cystitis or the ulceration in the bladder is sufficient to account for the amount of pus found in the urine. The appearance of the ureteral orifices is also indicative of the condition of the kidneys. A normal ureteral orifice is incompatible with a long-standing pyelonephritis with discharge of pus through the ureter. As a rule, the ureteral orifice is edematous, congested, or positively inflamed, and the region of the ureter is the site of either inflammation or ulceration. I do not believe that cystitis and ulceration, associated with tuberculosis of the kidney, can be differentiated from that due to infection from other organisms by means of the cystoscope alone.

Catheterization of the ureters, a study of the urine obtained from each kidney, and the injection of urine from each kidney into guinea-pigs, are the most reliable means of diagnosis at our disposal. The objection has been made to this method of diagnosis, that it involves the possibility of infecting the previously healthy ureter and giving rise to ascending infection. In no case has this occurred in my experience, either in tubercular or non-tubercular cases. To avoid, or at least to lessen the risk of infection, the bladder should be irrigated immediately before the examination; and when the supposedly healthy ureter is catheterized, the orifice of the ureter should be washed with a pledget of cotton immediately before inserting the ureteral catheter. When ureteral catheters are inserted to obtain urine for examination, it is unnecessary to pass them more than one or two inches, and in the supposedly healthy ureter it is best to be satisfied with a moderate insertion of the catheter.

Condition of the Bladder. The condition of the bladder before operation and the result of nephrectomy upon the condition of the bladder, is one of the most important subjects connected with tuberculosis of the kidney.

From the patient's standpoint, the irritability of the bladder, entailing the necessity for frequent urination, is the chief trouble for which relief is sought. Of the 10 patients upon whom I have performed nephrectomy for tuberculosis, in 3 the bladder was not examined. Two of these were emergency cases, seen in a neighboring town, whose condition was such as to demand operation on the day when they were first seen. In the third patient the bladder was not examined, as the operation was undertaken with a diagnosis of movable kidney. In 1 of the remaining 7 cases, bladder symptoms were not complained of, although there was some pus in the urine. A cystoscopic examination showed that the bladder was practically normal. Of the remaining 6 cases, cystitis was present in all, and ulcerations, more or less marked, were present in the region of the ureters and trigone in 5.

The cystitis and ulceration underwent a spontaneous cure in 5 of the 6 cases. In 1 case it was necessary to make topical applications to the ulcers of silver nitrate through the cystoscope. In 1 case a vesicovaginal fistula had been made prior to the nephrectomy, and it was not closed until the ulcers healed. In 2 of the patients, irrigation and distention of the bladder by gravity was practised to increase its capacity. This experience indicates that after nephrectomy for tuberculosis a secondary cystitis will usually undergo a spontaneous cure, which may be hastened in certain cases by irrigation, topical applications, or drainage. In the graver cases, complicated by extensive ulcerations in the bladder, the formation of bands or septa and a permanent loss of the full capacity of the bladder are to be expected in a portion of the cases.

Operation. Nephrectomy alone was performed in 7 of the 10 cases. Nephroureterectomy was performed in 2 cases, and in 1 the upper two inches of the ureter were removed, together with the kidney. Lumbar nephrectomy was performed in the last 9 cases. In the first, nephrec-

tomy by abdominal section, with a T-shaped incision, was the method selected. In this case the tuberculous abscess of the kidney was complicated by a large perinephritic abscess which had been drained four weeks before the radical operation.

Nephrectomy is a much simpler operation than nephroureterectomy, and is the operation of choice, unless the ureter is extensively diseased. It must not be forgotten, however, that inflammation of the ureter complicating tuberculosis of the kidney is frequently simple and not tuberculous. In 2 cases, when the ureter was known to be diseased, ureterectomy was not performed, because of the prostrate condition of the patient. In 1, the upper two inches of the ureter were removed, and in the other, no effort was made to remove ureteral structure. The most urgent indication for ureterectomy is tubercular involvement of the ureter with a stricture toward its vesical end. To leave the ureter under such circumstances would almost inevitably result in a permanent fistula.

The influence of nephrectomy, as compared with nephroureterectomy, in the healing of the wound is suggestive. Of the 10 operations, 6 were followed by sinuses in the lumbar wound, which remained open for varying intervals of from four to fourteen months. Both patients upon whom nephroureterectomy was performed developed sinuses. The sinus in the first case closed in about four months. In the second case a sinus formed twice, and closed finally about nine months after the nephroureterectomy. Of the remaining cases, the 2 in which the sinus remained open the longest were those in which the tuberculous process was most extensive. In 1 there was a perinephritic abscess with tuberculous infection of the entire region. In this case silk ligatures were used (1895), and the sinus did not close finally until all the ligatures had been discharged after about one year. In the second, a tuberculous ureter was left in because of the feeble condition of the patient. This sinus closed after fourteen months.

It is clear that a strong tendency exists for the persistence of a sinus following nephrectomy, whether or not all apparently diseased structure, including the ureter, is removed. This is doubtless due to the tuberculous infection of the perirenal tissues, recovery from which, like that from tuberculosis in general, is due to improvement in the patient's nutrition.

Result. Nine of the operations were done upon women and 1 upon a man; 2 were colored and 8 were white. Six of the patients were in bad condition, 3 in fair condition, and 1 in good condition at the time of operation. The man died, not from the nephrectomy, but, some six weeks later, presumably from involvement of the lung, and in the course of the tuberculous disease. The 9 women not only recovered from the operation, but all of them are in good health, also, at the present time. Most of them have gained from twenty to forty pounds in weight. All have been heard from during the preparation of this paper, and report themselves as well, with the exception that 3 have bladder symptoms. In 2, the bladder will not hold more than six ounces, and when this amount has accumulated urination is necessary. The third (operated upon in February of this year) is still obliged to urinate frequently. She has, however, improved locally, and has gained nineteen pounds.

The Incision for Nephroureterectomy. In the first nephroureterectomy the incision recommended by Kelly was used. In the second, after freeing the kidney and ligating and detaching its vessels, the ureter was detached as far down as feasible, through the lumbar wound, and then the patient was turned upon her back, the kidney being permitted to lie upon the operating-table, attached to the ureter. An incision was now made just internal to the outer border of the rectus muscle, that is, just internal to the semilunar line. The incision was made through the rectus rather than through the external oblique, internal

oblique, and transversalis, to avoid permanent damage to the abdominal wall. It is well known that the usual incision through these three muscles must lead to permanent injury of the abdominal wall. At the best, the muscles are tied up in a scar which is more or less uncomfortable when the abdominal muscles are brought into play, and at the worst, ventral hernia is invited. By making the incision through the rectus muscle, when the wound is closed the fascias can be overlapped, thus preventing hernia; and, in addition, the abdominal muscles are not tied up in a cicatrix, which must permanently interfere with their normal freedom of motion. The ureter can be reached as easily through this incision as through the one heretofore employed.

In each of the 10 cases of nephrectomy forming the basis of this paper, the tuberculosis of the kidney, presumably, was the primary lesion, and in only 1 (the colored man who died some six weeks after operation) were the lungs involved. As seen by the surgeon, tuberculosis of the kidney is usually unilateral. When both kidneys are involved, the tuberculosis of the kidneys is usually a part of general tuberculosis, or the second kidney becomes involved at a much later period than the first. In either case, the general condition of the patient is usually such that he is not referred to the surgeon.

The excellent results secured in this series of cases may be regarded as typical of what can be accomplished in the cure of tuberculosis of the kidney by operation, if nephrectomy is performed reasonably early in the course of the disease. To accomplish the most satisfactory results, nephrectomy must be performed before secondary involvement of the bladder has become extensive and before the disease has been disseminated throughout the body. These fundamental facts should be appreciated by the family physician. Were it feasible for him to make a positive diagnosis early in the disease, he should be urged to do so, and to advise

his patients to submit to early operation. Owing to the difficulties of positive diagnosis early in the disease, it is unreasonable to expect the physician who is not trained in the making of cystoscopic examinations and the catheterization of the ureters to make a positive diagnosis in early cases. He should, however, be on the alert to suspect the presence of tuberculosis of the kidney whenever he has to deal with a persistent pyuria with acid urine, and when the usual treatment for cystitis pursued for a reasonable time fails to effect a cure. In these and other cases of pyuria beyond his diagnostic resources, he should call in expert assistance in order that the diagnosis may be cleared up.

TABLE OF NEPHRECTOMIES FOR TUBERCULOSIS OF THE KIDNEY.

| Name and date. | Age | Condition. | Diagnosis. | Operation. | Ureter. | Union. | Result. | Family physician. | Bladder. |
|---|---|---|---|---|---|---|---|---|---|
| Mrs. B., Oct. 17, 1895. | 34 | Anemic, prostrated. | Tuberculosis of left kidney; kidney structure destroyed, perinephritic abscess; bladder not ulcerated. | Left nephrectomy; incision of perinephritic abscess; Sept. 17, 1895. | Not removed. | Sinus closed after 1 year. | Cured. | Klein. | Not involved. |
| Miss E., May 16, 1896. | 37 | Septic, bad. | Tuberculosis of left kidney; bladder ulcerated. | Left nephrectomy. | Not removed. | Primary. | Cured. | Minnich. | Cystitis and ulceration of bladder recovered spontaneously. |
| Mrs. L., Dec. 3, 1896. | 31 | Fair. | Tuberculosis of right kidney; bladder not examined. | Right nephrectomy. | Upper two inches removed. | Sinus closed in 6 months. | Cured. | Applebach. | Not involved. |
| Mrs. F. S., April 14, 1899. | 37 | Septic. | Tuberculosis of left kidney; bladder not examined. | Left nephrectomy. | Not removed. | Primary. | Cured. | Sharpless. | Not involved. |
| Miss E. B., July 24, 1899. | 30 | Bad. | Tuberculosis of right kidney; bladder ulcerated. | Right nephrectomy. | Infiltrated; but not removed on account of patient's condition. | Sinus closed in 14 months. | Cured. | Jones. | Ulceration treated topically through cystoscope. Cystitis treated by irrigations. Recovery, with the formation of a band or septum across bladder. Capacity of bladder increased from 60 c.c. to 180 c.c. |
| Mr. F., April 9, 1901. | 44 | Septic, very bad. | Tuberculosis of right kidney; bladder not examined. | Right nephrectomy. | Not removed. | Sinus. | Died of pulmonary tuberculosis. | Sharpless. | Not involved. |
| Miss C., Oct. 16, 1901. | 27 | Good. | Tuberculosis of right kidney, bladder not ulcerated. | Right nephrectomy. | Not removed. | Sinus closed in 6 months. | Cured. | Robertson. | Mild cystitis, recovered spontaneously. |
| Miss B., Oct. 20, 1902. | 27 | Bad. | Tuberculosis of left kidney; bladder ulcerated. | Left nephrectomy. | Not removed. | Primary. | Cured. | Clark. | Ulceration healed, but bladder remains contracted. |
| Miss C., June 15, 1905. | 22 | Fair. | Tuberculosis of left kidney; bladder ulcerated. | Left nephrectomy. | Removed. | Sinus closed after 4 months. | Cured. | Jones and Kyle. | Ulceration healed spontaneously. |
| Miss B., Feb. 14, 1906. | 25 | Fair. | Tuberculosis of left kidney; bladder ulcerated. | Left nephrectomy; cystotomy. Fistula subsequently sutured. | Removed stricture above vesical wall. | Primary. Sinus formed twice subsequently. | Cured. | Ingalls and Stanton. | Ulceration healed after drainage and irrigations. Bladder remains contracted. |

## DISCUSSION ON PAPERS OF DRS. KELLY AND NOBLE.

Dr. Edward McGuire, of Richmond, Va.—I wish to emphasize one point in Dr. Noble's paper, and that is the importance of repeated urinary examinations in suspected cases of tuberculosis of the kidney.

I had a case of tuberculosis of the kidney, several years ago, which impressed the importance of this point on me. I advised an operation, but the family were not satisfied in regard to the operation, so I referred the case to Dr. Osler for consultation. She consulted him; the ureters were catheterized and urinary examinations were made, also inoculations of guinea-pigs. Dr. Osler wrote me, some weeks after, that in his opinion it was not a case of tuberculosis of the kidney and that possibly the pathologist had mistaken the smegma bacillus for the tubercle bacillus.

I saw her about six months later and, still thinking it tuberculosis of the kidney, advised its removal. She then determined to consult some one else and went to Philadelphia and saw one of the most eminent internists there. He referred the case to a specialist; analyses and injections were made. He reported to me at first that he did not find the tubercle bacillus, but subsequently wrote me that the guinea-pig which was injected had died of tuberculosis.

I think it is possible that the tubercle bacillus might not be found in one specimen while another would demonstrate its presence. Certainly, this case emphasizes the importance of making repeated urinary examinations.

I also wish to emphasize what Dr. Crile has said with reference to late operations in tuberculosis of the kidney. I recall operating on a case about six years ago. Personally, I was very much interested in the case. From examinations made the kidney was destroyed and there was tuberculosis of the bladder and also of the lung, and I must confess that I thought an operation would do little or no good. I refused to operate and told her that if she would consult Dr. Osler and he advised operation that I would do it. Dr. Osler advised against operating and wrote me that he thought she had but a short time to live. She still insisting that I should do something for her, I told her to go down South and that if she gained ten pounds I would operate on her, never thinking that she would improve. When she had gained ten pounds she came to the hospital without notifying me, fearing

that I would still refuse to operate on her. I removed the kidney and ureter and her condition commenced to improve at once. Since the operation she has been living out of doors and spending most of the time in the Adirondacks. She states that her physician there says her lungs are cured. She still has some slight bladder symptoms but weighs more than she ever did.

Dr. Robert T. Morris, of New York.—I would like to make one point supplementing Dr. McGuire's remarks. Some time ago I experimented, with the idea of ligating the ureters in tuberculosis of the kidney, for the purpose of setting up a leukocytosis of absorption in the hope of wiping out tuberculosis. Accordingly, I made injections into the kidneys of a series of rabbits of test-tube cultures of tuberculosis. In some cases the cultures were injected just beneath the fibrous capsule of the kidney; in others the injections were made into the parenchyma of the kidney. At the end of a few weeks I opened some of these rabbits to see how the cultures were getting on, preparatory to ligating the ureter. I found my cultures well under way, and there were well-marked masses of tubercle deposit. I waited a few weeks longer, and was then ready to ligate all of the ureters. I opened the rabbits and every single one of them had gained control of the tuberculosis, so that it had almost disappeared. That is an important point. When we have some tuberculosis left we have to remove this focus of infection, by increasing the natural resistance of the patient, and in some cases we are able to wipe out the remaining tuberculosis.

Dr. I. S. Stone, of Washington, D. C.—With reference to the paper read by Dr. Kelly, we must admit that, as a rule, his premises are correct. But it has a doleful sound when applied to a patient who has both kidneys infected or involved. There is some benefit, however, to be derived from the "outdoor" treatment in these cases of tuberculosis of the kidneys, although Dr. Kelly has not suggested very much in that direction. Eighteen months ago a patient, with severe cystitis, came under my observation. After many tests with the microscope and other means, such as injecting guinea-pigs and using the tuberculous test, it was found she had tuberculosis of both kidneys. Her bladder was ulcerated around the orifices of the ureters. She has gained considerably in weight by living in the open air and by partaking of good, wholesome food, and plenty of it. At the present time she has very little irritability of the bladder, and her condition is extremely satisfactory. I do not know how we are to treat

such patients in any other way than by the simple method of giving good food and plenty of fresh air. For them surgery can do nothing. For the most part I am in accord with the statement made by Dr. Noble, that the diagnosis in these cases should generally be made with the aid of the cystoscope. However, I do not want all of you to think because a patient has redness or a little inflammation around the openings of the ureters that she has tuberculosis; I will tell you why. I recently did a nephrectomy for hypernephroma in a patient whose ureteral orifices were very much reddened and swollen. There was no tuberculosis in this case, but in looking at that part of the bladder one might have thought so. It is not necessary, therefore, that this rule should apply in every case.

Dr. Mack Rogers, of Birmingham, Ala.—I wish to reiterate what has been brought out in these most excellent papers with reference to the removal of tuberculous kidneys, and think it should be done just as soon as we establish the diagnosis.

This infection is usually unilateral, and secondary to a similar lesion elsewhere, and this is most often found in the lungs.

Bearing in mind, then, the frequency of pulmonary tuberculosis, when our patient complains of trouble with the urinary apparatus, we should, among the first things, suspect that the disease has attacked the kidney, and we should direct our efforts along that line until we have settled the question.

And I know of no method that compares favorably with the skilful use of the cystoscope and ureteral catheter. The cystoscope will reveal the condition of the bladder mucosa, and the catheter will give unmixed urine from the suspected organ. I have ceased to share in the great horror we have been entertaining with reference to catheterizing the ureters, for I believe with the present perfect aseptic technique we all practise now that there is very little danger in making this observation, and I am convinced that a single such observation is not sufficient upon which to base a negative opinion; therefore, my rule is to make at least three of these examinations, say four or five days apart, and if there are any bacilli present we are apt to find them, and once they are detected we should repeat our observation to confirm our diagnosis.

And, now, with reference to dealing with the ureter after the kidney has been removed. The simple ligation of the stump has not been altogether satisfactory, and since we may fairly suspect ulceration of the ureteral mucosa, the practice of injecting

a drachm of pure carbolic acid into the ureteral stump and then ligating it has given such good results that I think it should be generally practised, for by this procedure the ulcerated open ureter becomes obliterated and shrinks to a mere fibrous cord, and the acid runs down the ureter on to the ulcers of the bladder mucosa and is there diluted by the urine in this organ, so it does no harm to the bladder and if this does not cause the ulcers to heal they must be treated locally by the cystoscope.

Dr. J. Wesley Bovée, of Washington, D. C.—There is one point in the diagnosis to which I wish to refer regarding the tubercle bacillus, but which I did not hear mentioned and that is, in many cases the tubercle bacillus is found in the urine in which there is no tuberculosis of the urinary tract; they are not cases of mistaken identification of the microörganism.

As to the advisability of removal of the ureter when it is involved to any extent, I think it was Reynier who had to do two partial ureterectomies after removing the kidney and part of the ureter. This was done on account of a persistent fistula. It is very essential sometimes to remove the ureter as well as the kidney. There are some cases where complete removal of the ureter seems inadvisable because of the condition of the patient.

I also wish to advise nephrotomy preceding nephrectomy, as Dr. Kelly advised in the dangerous cases, particularly in those cases that are very much prostrated by the progress of the disease. Nephrotomy can be done quickly in these cases and gives great relief. When the patient has improved sufficiently nephrectomy can be done, and perhaps nephroureterectomy. In two of my cases I followed that course; I let out the pus, and after the patient had improved I did a nephroureterectomy.

I do not think we can safely recommend the passage of carbolic acid through the ureter, as Dr. Rogers has mentioned, with a view to staying the process of tuberculosis along the ureter.

As regards the method of operation in women, I believe, as I suggested a few years ago, that the best method of complete nephroureterectomy is to make a vaginal incision along the ureter, separating the bladder, and a transverse incision for liberation of the kidney and upper part of the ureter. This transverse incision will then permit of the removal of the kidney and the ureter, and bring it all out from above in a much shorter space of time. I first saw this done by Dr. Montgomery in a case of nephroureterectomy which he reported. I was much impressed with the rapidity with which he did the operation and I followed

that method afterward. He modified it, however, by doing the vaginal work afterward; but the vaginal work, if done first, is much more favorable.

I hope Dr. Hunner will tell us about the nephrouterocystectomies Dr. Kelly has done, and whether he does partial resection of the bladder or a real cystectomy. So far as I am aware, cystectomy, with transplantation of the ureters, has been done in 4 cases for tuberculosis.

Dr. Henry T. Byford, of Chicago.—I do not know whether it is in order to speak of methods of operating or not, but I wish to say that in my early cases I operated six times transperitoneally with good results except in 1 case; the woman recovered from the operation, but died later of uremia. I sewed the retroperitoneal wound in 1 case only. I omitted ligating the ureter in 1 case without any bad effect. I drained invariably with gauze leading out through a knife puncture in the loin, removing it in from twenty-four to thirty hours. It appears to me, that this should be the operation of choice in many cases as it is attended with very little danger; there is less traumatism inflicted on the tissues, and there is less probability of infection of the parts. An opportunity is afforded for an examination of the other kidney.

Dr. Hunner (closing the discussion for Dr. Kelly).—Before answering the discussions concerning Dr. Kelly's paper I would like to comment on some of the points mentioned in the papers of Drs. Kelly and Noble.

Concerning the prognosis, when both kidneys appear to be diseased, we should not be too ready to conclude that both kidneys are diseased even when careful investigation seems to point to this conclusion. I remember distinctly a young woman, nineteen years of age, who consulted me about two years ago. She had severe bladder symptoms, elevation of temperature, pain in the left side, and enlarged and tender left kidney and ureter. The left ureter was so thickened near the bladder that a renal catheter could not pass. On palpation, near the bladder, the right ureter was indurated and an attempt to catheterize this side was likewise unsuccessful. Tubercle bacilli were found in the urine, and I concluded that both sides were diseased. The symptoms called for interference on the left side, and fearing to do a nephrectomy without a definite knowledge of the conditions on the right side I decided to do left nephrotomy. On delivering the kidney I found that the disease was almost entirely confined to the upper pole. I therefore did partial nephrectomy, but was forced to

leave evident disease in the lower half of the kidney. I also made a vesicovaginal fistula for drainage. The patient improved rapidly, but after a few months the left kidney again gave symptoms calling for surgical interference.

Before removing the remnant of this kidney I performed nephrotomy on the right side, and was surprised to find no macroscopic evidence of tuberculosis. Sections from different portions of this kidney were negative microscopically. After recovery from this operation I removed the remaining half of the left kidney. Instead of nephrotomy on the right side I should have made another attempt to catheterize, and would probably have been successful, after the improved conditions in the bladder region. I closed the vesicovaginal fistula, and the patient now weighs 165 pounds, but I am still treating her for a minute ulcer in the left base of the bladder.

Dr. Kelly spoke of having 36 cases of tuberculosis of the bladder in his list of 62 cases. I think we should use the term "tuberculosis of the bladder" advisedly, as many patients with tuberculosis of the kidney have bladder lesions which are not tuberculous. Removal of some of these ulcers and microscopic examination have shown that they are simply inflammatory and non-tuberculous. In other cases the bladder lesions have been untreated and have quickly healed after removal of the diseased kidney. In my judgment, any ulcer that heals quickly and spontaneously it not of a tuberculous nature.

In reply to Dr. Bovée, we should use the term "partial cystectomy."

Do not give a patient or his relatives too serious a prognosis if you have removed one tuberculous kidney and know that the remaining kidney is diseased. I remember a patient in the private ward at the Johns Hopkins Hospital, in 1894, from whom Dr. Kelly removed a tuberculous left kidney. At that time he had evidence of pulmonary tuberculosis and disease in the right kidney. Two years later he returned for operation for double orchitis. At this time his lung condition was worse and Dr. Halsted insisted on his dropping his profession as teacher and taking up an outdoor occupation. He bought a sheep farm, and after one year of outdoor life he weighed 180 pounds and felt so well that he again took up his work as teacher of chemistry, devoting a half day to this and a half day to farming. During the past winter he was holding farmers' institutes, travelling from town to town in all sorts of weather.

Dr. Noble, in his paper, spoke of the difficulty in making a diagnosis except in the late cases. I do not think we should disseminate that information or convey that impression. We ought to have the general practitioner feel that he should be able to make the diagnosis in the early cases. Dr. Noble further said, that pain in the kidney is one of the first symptoms. If we wait for pain in some cases, we will wait until both kidneys are diseased; in fact, patients come to us with both kidneys diseased who have never had kidney pain. There are usually symptoms of cystitis from the first. Any case of cystitis, with pus exhibited in the urine, may be suspicious of tuberculosis until the case has been proven to be something else; especially if the urine is acid, and a culture of it on ordinary media is negative, you may suspect tuberculosis.

One of the speakers asked regarding the post-operative treatment of these tuberculous bladders. As I have remarked, many of them are not tuberculous. If after the removal of the kidney the patient is comfortable, I think it well enough to leave her alone and not treat the bladder; but if the bladder is not well at the end of a year then treat it, because there is probably a tuberculous ulceration. If there are bladder symptoms after operation, all cystoscopists are agreed that bichloride instillations form the treatment *par excellence* for bladder tuberculosis.

Dr. Noble spoke of overdistending the bladder. I think he probably means after the tuberculosis has healed and the bladder is still contracted. There is no objection to that, but most of the workers in this line are convinced that it is harmful, in tuberculous cases, to bring about overdistention of the bladder, and that is why they do not irrigate such bladders, but use small instillations of a strong solution. The overdistention should not begin until the mucosa is healthy. As to the strength of the bichloride solution, it should be as strong as the patient can stand —say 1 to 5000. The German physicians use 1 to 2000.

Dr. Jordan.—Have you ever found it necessary to perform vaginal cystotomy for the relief of the bladder condition?

Dr. Hunner.—Yes! we have done it in a number of cases.

The case cited by Dr. McGuire in which there was temporary disappearance of the tubercle bacilli is a most instructive one. Repeated urinary examination should be made in these cases. One may not find the tubercle bacilli today, but by repeated daily examinations they will be found in the course of a week or two. According to my experience we ought to find the tubercle

bacillus in 80 per cent. of the cases that have tuberculosis of the kidney. One of my patients will leave the Johns Hopkins Hospital to-morrow, one month after the removal of her right kidney. On her admission to the hospital, about three months ago, catheterization of both kidneys showed tubercle bacilli in both sides. The urine from the left side, however, showed but the smallest amount of pus and very few bacilli. Fearing that some mistake may have crept in, I again catheterized this side a few days later, and again found the same conditions. After treating the bladder for some weeks and giving the patient outdoor treatment, intending to send her South for the winter, I again catheterized both sides; the urine from the right side was decidedly worse and loaded with tubercle bacilli, while the urine from the left side was absolutely normal, containing no pus, bacilli, or other evidence of inflammation. The chances are that if this patient could be examined at some later day, we would find another focus of infection making itself apparent on the left side.

Dr. Bovée advocates the vaginal incision for removal of the ureter. We never know how difficult the lumbar nephrectomy is going to be, and it does not seem wise to begin a possibly serious operation by work in the vagina, especially since the removal of the ureter is never absolutely necessary at the first operation. When removal of the ureter seems advisable at the first operation, I remove it through a low McBurney incision and always find that this gives ample room. After the ureter is removed close to the bladder I make a vaginal puncture at this point and leave vaginal drainage, closing the McBurney incision.

Dr. Noble (closing).—It is quite evident that my language was obscure or my reading was very bad, because a number of speakers quite agree with what I said in the paper. For instance, Dr. Stone brought out the point of differentiating between ordinary cystitis, or ulceration, and tuberculosis with the cystoscope. I quite agree with him, and also with what Dr. Hunner said, with reference to early diagnosis. We should aim to make an early diagnosis. When the family doctor finds pyuria, with acid urine, in a case, he should be suspicious. What I said in my paper in regard to this, was that we could not expect the general practitioner to catheterize the ureters, make cultural tests, etc., and so in these cases he should call in expert advice.

As to the statement made by Dr. Rogers, that the lungs are usually involved before the kidney, I do not think that is true. When we say that tuberculosis is primary in the kidney we

mean there is no other focus determinable in the body; and whether the infection came through the lungs or from the intestine, we do not know. The point is, if we can find the only focus in the body we speak of it as a primary tuberculosis, and the germs must reach the kidney through the blood—hematogenous infection.

In reference to pain as an early symptom, that is another point concerning which I must have been obscure; I think pain is a late symptom, and an index, as a rule, that the kidney is extensively involved. In only 1 case was it observed early in my experience.

Concerning preliminary nephrotomy, I think it is a mistake to do many of these operations. As a rule, unless the condition of the patient is desperate, I believe it is better to do a nephrectomy as the primary operation. If we have perinephric abscess, or the condition of the patient is desperate, then nephrotomy is permissible. I am quite satisfied that the number of nephrotomies will become less as experience increases.

In regard to Dr. Byford's suggestion to do transperitoneal nephrectomy, I would not be inclined to it, particularly in a case where we have a large abscess of the kidney, as there would be risk of rupturing such a kidney, with the dissemination of the tuberculous pus through the peritoneal cavity, and this should be avoided. If we can manage these cases satisfactorily through a lumbar incision, I much prefer to do it.

The only other point which I will allude to is with reference to the second incision for nephro-ureterectomy. I think it is a mistake not to give one's self enough room. I had one accident in doing a ureterectomy through a small incision, where I relied on touch particularly instead of vision, which impressed on me, very forcibly, the desirability of having plenty of room; that is to say, the ligature work was all done satisfactorily until I cut the ligature off the ureter—when I cut the external iliac artery with scissors and had to ligate it. I think it is a mistake to try to do that work through a small incision. That is what suggested to me the advantage of making an incision through the rectus muscle, because then you can make it as long as you choose without damaging the abdominal wall and avoid accident. It certainly facilitates surgical work deep in the pelvis to have plenty of room in which to work.

# A STUDY OF VAGINAL CESAREAN SECTION:

## CONSIDERED WITH ESPECIAL REFERENCE TO SUBSEQUENT PREGNANCY AND LABOR.

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There is perhaps no subject in the domain of obstetrics that has given rise to more spirited controversy than the status of vaginal Cesarean section. Dührssen,[1] who broadened its indications and developed its technique, is very optimistic as to its applicability. Pozzi regards it as the last great surgical feat of the nineteenth century, and that no more valuable operation since the introduction of forceps has been introduced into midwifery, and De Lee[2] believes it constitutes the most important addition to obstetric technique contributed in the past thirty years. The literature in support of the operation is growing steadily; a number of congresses have regarded it favorably, and the one at Kiel,[1] in 1905, recognized its superiority over all other methods of vaginal delivery in closed or degenerated cervix.

Like many innovations, however, the operation has been vehemently opposed as being an irrational, hazardous procedure, and even stigmatized as the prostitution of surgery. Bossi,[3] the champion of instrumental dilatation of the cervix, deprecates it and says it should be abandoned. Ahlfeld,[4] Hofmeier,[5] and others contend that it is unnecessary, claiming that equally good results can be obtained by more conservative measures. Palmer Findley[6] says its only justification is possibly in rigid cervix; otherwise, if a cutting

operation is elected, the classical method, on account of precision, asepsis, and expediency, should be given the preference. As a matter of fact the truth of the question probably lies between these extreme views. To appreciate the sphere of each of the various methods of artificial cervical dilatation, for each has its specific indication, one must have in mind the anatomical changes and the physiological action of the uterus during pregnancy and labor. In the primipara the cervical canal is closed throughout pregnancy; while in the multipara the internal os may be closed; the external is patulous. In the latter months of gestation in the primipara, owing to the increasing intra-uterine tension on account of the intermittent contractions and resistance of the cervical barrier, the passive lower segment is formed; and at or just prior to labor the forces are also exerted upon the internal os, which gradually unfolds until complete dilatation of the cervix is effected. In the multipara, however, because of the relaxed condition of the uterus, the intra-uterine pressure is not so great except in hydramnios and twin pregnancy; so that there is little effacement of the internal os during labor, and in dilating the cervical canal assumes rather a tubular form, unlike that of the primipara which is conical.

Since vaginal Cesarean section is not subject to such restrictions and the operation is indicated, according to Dührssen, when the cervix is intact or imperfectly dilated and not yielding to gentler measures, and the life of the mother or child is in danger, as from pathological conditions of the lower segment, stenosis, acute toxemia, eclampsia, antepartal hemorrhages, heart and lung complications.

Dührssen[7] has collected 378 cases of vaginal Cesarean section with 48 deaths, or 12.7 per cent. He attributes this high mortality not to the operation, but to the serious conditions under which the operations were undertaken. In 53 of the cases the uterus was extirpated after the vaginal Cesarean section; only 5 died, 9.4 per cent.

In this connection it may be truthfully stated that general statistics are not always infallible as to the merit of a measure in any particular case; depending, as they do, upon environment, judgment, and skill of many operators. Rather should we judge of its possibilities from the results obtained by the individual operator. For instance, in 33 cases of Cesarean section for eclampsia, Veit had only one death, and Bumm had only one in 40 consecutive cases.

The principal objections advanced against the operation are: (1) injuries to the bladder and danger of extension of the incision and opening into the peritoneal cavity; (2) liability to hemorrhage and sepsis; (3) rupture of the uterus in a subsequent labor. The first should be obviated by careful technique. If the child is premature an anterior incision will provide sufficient opening for its passage. At term, however, it may be necessary to incise the posterior wall also; although Liepmann delivered a child weighing 3950 grams, in which only anterior hysterotomy was performed. Further, in more than 60 cases operated upon by Bumm and Liepmann, by the same method, none were torn beyond the incision. The circumference of the child's head at term is 30 to 34 cm., so that the opening of the cervix must be from 8 to 10 cm. in diameter; sufficient to permit of the passage of a large-sized fist. In a narrow vagina, deep vaginoperineal incisions may be necessary to facilitate rapid and safe delivery. The danger of infection is no greater than in the use of the tampon, metreurysis, forceps, version, manual or instrumental dilatation. Hemorrhage has not infrequently occurred, but it was probably due to inertia and no fault of the operation. In a study of 140 cases by Stamm,[8] there was no alarming hemorrhage and only 2 of infection. In the 248 cases reported by Dührssen[1] there is not a fatal result considered as directly due to hemorrhage. Dührssen's contention, however, that asepsis can be brought about in just as an exact a manner in vaginal hysterotomy as by the classical method is not

supported by the light of bacteriological investigation, which has demonstrated that, while the uterus and vagina may be free from pathogenic bacteria, the vulva is rarely ever so, and it is almost impossible to render it sterile. Neither is his claim that, with the proper technique, the child is not any more endangered in vaginal delivery than by the abdominal route, in accord with the experience of most authorities. The claim of liability to rupture of the uterus in subsequent labor is not supported by the cases thus far recorded. In searching the literature, and with the assistance of Professor Dührssen, I have been able to collect 11 cases of this variety, to which I add 1 of my own. These are briefly stated as follows:

Dührssen (*Handb. der Geburtsh.*, v. Winckel, 1906, Band iii, Teil. 1), relates a case in which, three years after vaginal hysterotomy for vaginal fixation, the patient became pregnant, and at term the child presented transversely. Complete cervical dilatation was accomplished in six hours by the Champetier de Ribes bag, and delivery effected by version and extraction.

Wennerström (*Centralbl. f. Gynäkol.*, 1903, No. 10). Frau K., multipara, aged twenty-eight years. Incarcerated, retroverted, pregnant uterus; cessation of menses five months. The usual methods of correcting displacement proving ineffectual, and menacing symptoms manifesting themselves, posterior vaginal hysterotomy was performed, and a macerated fetus 15 cm. in length was removed (about three months gestation). Two years later the patient had a normal birth.

Stamm (*New York State Med. Journ.*, 1906, vi). Vaginal Cesarean section for eclampsia in September, 1903. In July, 1904, she gave birth to a seven and one-half months' child before the arrival of a physician, and without any untoward result.

Westphal (*Centralbl. f. Gynäkol.*, 1906, No. 29). Frau X., II-para, aged twenty-three years. Eclampsia. Unsuccessful application of conservative methods; vaginal

Cesarean section performed. Postpartum hemorrhage controlled by tampon. On account of threatening collapse, exact suturing of incision was not possible; but the result, however, was excellent. Twenty-four hours afterward the convulsions had ceased, mind cleared, and on the following day albumin had disappeared from the urine. Examination three weeks later revealed a small granulating surface of the anterior fornix and cervix. Four months after operation, again pregnant two and one-half months; edema of face and limbs; albumin, 2 per cent. Aborted at three months. Some weeks later, genitalia and kidneys normal. In May, 1905, became pregnant for the third time; no disturbance of kidneys; gestation progressed normally to an easy birth without assistance February 21, 1906.

Rühl (*Münch. med. Wochschr.*, 1906, liii, 507). Frau X., November 19, 1903, primipara, aged twenty-five years. Anterior vaginal Cesarean section for rigid os. During extraction a transverse tear of the lower segment extending from incision into the parametrium occurred; recovered. Saw patient again at the eighth month of pregnancy. Examination: vertex presentation, first position; extending into left parametrium, a dense cicatrix was felt. He feared the delivery would be attended by great danger; but, much to his surprise, the birth was easy, of five hours' duration, and the child was living. The placenta was expelled spontaneously; puerperium normal.

Rühl (*Ibid.*). Frau X., aged twenty-nine years. At term, July 12, 1903; R. O. A. Vaginitis; rigid os; pains severe but ineffectual; waters expelled after three days; unfavorable symptoms developing; temperature 39.3° C., pulse 125. Vaginal Cesarean section was elected on account of expediency; child living; puerperium morbid and recovery slow. Patient returned two years later and was three months pregnant. Examination: uterus was drawn to left side and fixed. The organ was freed by opening parametrium and cutting through adhesions. Abortion

was expected but pregnancy continued to term; and labor continued to term; child living; puerperium was normal.

Rühl (*Ibid.*). Frau X., primipara. December 5, 1903, eight months' gestation; eclampsia; edema of feet; albumin marked. Anterior vaginal hysterotomy was performed. One convulsion after operation; puerperium normal. The next pregnancy was normal, terminating with an easy labor of six hours' duration. Adherent placenta removed manually three hours after delivery; puerperium was normal.

Rühl. Frau X., multipara, aged thirty-one years. Labor began January 15, 1904; waters expelled twenty-four hours later; in spite of all-day pains, cervix failed to dilate beyond diameter of finger; colpeurynter introduced, with no increase in dilatation, after eight hours' continuous traction. The bag was removed and the case allowed to proceed without interference, until the 21st, when the temperature rose to 38.9° C., pulse 118. Anterior vaginal Cesarean section was performed; child alive; puerperium normal. At the next labor, early expulsion of waters; four days' strong pains without dilatation; hot douches, atropia, morphine, etc., were given; and at the end of six days only three fingers could be passed through the cervix. The Bossi dilator was introduced, and during thirty-five minutes was screwed up to 8 cm., when a hemorrhage suddenly occurred, necessitating removal of instrument and immediate version. Extraction progressed slowly, as far as head, when multiple cervical incisions were necessary to terminate the delivery. Placenta expressed, and postpartum hemorrhage controlled by gauze packing. There was an injury to the bladder which closed spontaneously in three weeks.

v. Bardeleben, reported from Liepmann (*Centralbl. f. Gynäkol.*, 1906, No. 46). Frau X., multipara, aged thirty-seven years. January 23, 1903, vaginal Cesarean section performed for placenta previa; six months' gestation. On examination, sixteen months later, was found to be four months pregnant. The subsequent labor was easy.

v. Bardeleben (*Ibid.*). Frau M., primipara, aged twenty-four years. On August 22, 1902, vaginal Cesarean section was performed for eclampsia; child living. Patient two years later had a normal pregnancy and a natural labor.

Jerie (*Handb. d. Geburtsh.*, Winkel, 1906, Band iii, T. 1). Frau X., primipara, aged twenty-three years. Eight months' gestation; eclampsia; sixteen convulsions; 5 per cent. albumin. Vaginal Cesarean section; child dead; mother recovered. Two years later, normal birth.

At the last meeting of the Association,[9] I presented by title a report of two cases of vaginal Cesarean section performed for eclampsia, both successful, and I now report a subsequent pregnancy and labor occurring in one of them. Mrs. F., white, aged twenty-seven years; I-para; six and one-half months' gestation. Admitted to Columbian Hospital, December 2, 1904, suffering with eclampsia. After five hours' treatment with morphia, etc., proving ineffectual, patient having had eight convulsions, coma deepening, and cervix undilated, anterior vaginal hysterotomy was performed and delivery terminated with forceps and placenta expressed; child lived thirty hours; puerperium normal. Saw the patient again, on account of threatening toxemia, in consultation with the attending physician, Dr. Ball, in November, 1905, soon after the beginning of her second pregnancy. Careful supervision of pregnancy was exercised, the untoward manifestations disappeared, and gestation continued to eight and one-half months. The preliminary examination revealed a justomajor pelvis, external conjugate 22 cm., spines 27.5 cm., crests 30 cm., child small, vertex presenting, first position. Labor began July 18, 8 P.M.; waters ruptured about midnight. The pains of the expulsive stage were controlled by chloroform to prevent precipitate labor and possible rupture of the hysterotomy cicatrix; the labor was easy and of five hours' duration; child living, weight six and three-quarters pounds; puerperium normal. Examination, November 11, 1906: uterus

in good position and freely moveable; fornix normal; general health excellent.

The number of cases is perhaps too small to form any definite conclusion as to the effect of the operation upon the uterus in subsequent labors; yet, from the outcome of those above related, it would appear promising. The writer believes the danger of rupture is averted by the passive state of the lower segment during labor favoring its integrity, and, further, the through-and-through suturing of the incision, at the time of operation, secures a firm union.

Everke[1] attributes the liability to rupture of the uterus, following the Sänger method, to the decidua not being included in the grasp of the suture; and that it is probable, that in the contraction and retraction of the uterus, the apposed sides of the incision become separated, forming a furrow of fibrous tissue covered by the peritoneum and mucous membrane. The writer has been able to verify this apparent change in a labor following the classical section.

It cannot be denied that complications and sequelæ attend vaginal Cesarean section; but that is also true of all the methods of accouchement forcé. Each has its value, and should be employed only after a thorough examination has determined its suitability. An exact knowledge of the condition of the cervix, before acting, is the essential prerequisite to success. The bougie, tampon, and metreurysis may be used when the cervix is undilated, provided time is not a necessary factor. Manual and instrumental methods should be applied only when the internal os is effaced or effacing. If applied while the cervix is intact, they act by divulsion rather than by dilatation, with its consequent dangers of hemorrhage, laceration, and infection. It is in the latter class that vaginal Cesarean section is particularly indicated, and further has the advantage that, in case of urgency, the uterus can be emptied at any period during pregnancy or labor in a given time.

It is, therefore, a meritorious operation and a valuable addition to our obstetrical resources.

References.

1. Handb. der Geburtsh., 1905, iii, Teil 1.
2. Chicago Medical Recorder, 1905, xxvii, pp. 599-611.
3. Centralbl. f. Gynäk., 1906, xxx, pp. 748–752.
4. Centralbl f. Gynäk., 1903, lii, No. 1.
5. Deutsch. med. Wochschr., Leipzig u. Berlin, 1906, xxxii, pp. 175–179.
6. Medical Herald, St. Joseph, Mo., April, 1906.
7. Surg., Gyn., Obstet., Chicago, July, 1906.
8. New York State Medical Journal, 1906, No. 6.
9. American Medicine, November 1906; Trans. South. Surg. and Gyn. Assoc., 1905

# DYSTOCIA FOLLOWING VENTRAL SUSPENSION AND FIXATION OF THE UTERUS.

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At the Birmingham meeting in 1904, Dr. Joseph Taber Johnson read a paper upon the "Effect of Suspensio Uteri upon Pregnancy and Labor," and some of you may remember that in the discussion following it I reported two cases of serious dystocia following that operation.

Those cases have since been reported in detail by my former assistant, Frank W. Lynch, and do not require elaboration at this time. Briefly stated, both patients were multiparous women, who had gone through several spontaneous deliveries prior to the operation, but had serious difficulty following it. In the first case, fixation had been done, which gave rise to severe pain and a backward dislocation of the cervix during the succeeding pregnancy, and labor had to be completed by manual dilatation of the cervix followed by a difficult version and extraction. In the second, firm fixation had followed a supposed suspension and gave rise to serious dystocia. At the time of labor the cervix was found to be at the level of the third lumbar vertebra and, as it did not dilate spontaneously and could not be dilated manually, Cesarean section was necessary.

Since then I have been unfortunate enough to see two other cases of the same character; one requiring craniotomy and the other Cesarean section.

I shall report these cases in detail, and then from my

experience and reading upon the subject I shall consider the complications to which these operations occasionally give rise at the time of labor, and, finally, state my views as to the justifiability of their employment in women during the childbearing period.

Case I.—Was seen in consultation with Dr. M. G. Porter, July 14, 1905, from whom I obtained the following history: Mrs. E., aged twenty-eight years, one spontaneous labor five years previously. Three years later the relaxed vaginal outlet was repaired and the uterus suspended, after which she was in excellent health. The last menstrual period was on September 21, 1904, and the pregnancy was uneventful except for almost constant, sharp, drawing pains in the lower abdomen. Vaginal examinations were made by Dr. Porter several times during the last month of pregnancy, but revealed no abnormality except that the cervix was so far back in the pelvis that its external os could be reached only with difficulty.

Labor began at noon and progressed normally until the membranes ruptured at 8 p.m. Examination at that time showed an oblique position of the child with the head in the left iliac fossa, a loop of pulsating cord in the vagina, and the cervix about two-thirds dilated with its lower margin opposite the promontory. Dr. Porter completed the dilatation of the cervix by Harris' method and attempted to perform version, but as he was unable to find a foot, gave up the attempt and sent for me. On my arrival at 9.45 p. m., I found a large woman with a very pendulous abdomen, which presented a markedly retracted longitudinal scar in its midline just above the symphysis. The pelvic measurements were normal, palpation was difficult, and heart sounds could not be heard, but the child apparently lay obliquely with its head in the left iliac fossa.

The patient was then anesthetized, placed upon a table, and prepared for operation. On vaginal examination a pulseless loop of cord was found protruding from the fully

dilated cervix, which was so displaced upward and backward that its lower or anterior margin lay opposite the junction of the first and second lumbar vertebræ. The superior strait in front of the cervix was filled out by a thick mass of tissue, which I assumed represented the hypertrophied anterior wall of the uterus. Had the child been alive, I should have insisted upon Cesarean section as the most conservative method of delivery, but as it was dead I determined to attempt its delivery per vaginam.

*Operation.* Owing to the abnormal position of the cervix, the hand could be introduced into the uterus only in a position of extreme flexion. The placenta was readily felt upon its anterior wall, and after careful palpation it was found that the child lay with its back posterior and its feet in the most dependent part of the pendulous uterus. Great difficulty was experienced in seizing a foot, but after repeated efforts the right was brought down to the vulva; even then version could not be effected, so that the result of my work was merely to have brought the right leg into the vagina in a position from which it could not be dislodged. For a time it seemed that delivery could not be completed, and I sincerely regretted that I had not performed Cesarean section, notwithstanding the fact that the child was dead. In desperation I tied a fillet to the foot, and had Dr. Porter make firm abdominal pressure in the hope of pushing the child's head near enough to the cervix to permit of perforation. Owing to the dislocation of the cervix and the extreme mobility of the head, craniotomy was extremely difficult, but was finally accomplished and the greater part of the skull removed in pieces, after which the child was extracted without great effort. I can, however, truthfully say that it was the most difficult delivery with which I have ever had to deal.

The child was very large and weighed 3500 grams (seven and one-half pounds) without the brain and greater part of the skull. The placenta came away spontaneously and examination afterward showed only a slight tear of the

cervix and perineum. The latter was immediately repaired and the patient made a most satisfactory recovery in spite of the prolonged manipulations. On examination three weeks later the uterus was found to be well involuted and in normal position, while a dense band of adhesions extended from the uterus to the lower angle of the abdominal cicatrix.

Case II.—M. N. (Obst. No. 2479), entered the obstetrical ward of the Johns Hopkins Hospital, February 7, 1906, and gave the following history: aged forty-one years, married eighteen years, twin labor with eclampsia in Germany, eleven years ago. Two years previous to admission the relaxed outlet had been repaired and the uterus suspended in Dr. Kelly's service. She menstruated last on May 22, 1905, and except for some nausea, which continued throughout pregnancy, complained of no untoward symptom.

Examination on admission and on several other occasions during the following four weeks, showed a normal pelvis and a large child lying in the left acromico-dorso-anterior position. In the midline of the abdomen, just above the symphysis, was a short cicatrix which showed no sign of retraction. The vaginal outlet was in excellent condition, while the bilaterally lacerated cervix lay far back in the pelvis and was somewhat higher than usual, but did not appear to be sufficiently displaced to give rise to serious dystocia.

Labor began at 9 A.M., March 5, 1906, and the membranes ruptured spontaneously at 2 P.M. Two hours later a vaginal examination was made by the resident obstetrician, Dr. Goldsborough, who found the cervix soft and voluminous with its canal obliterated and the external os 3 cm. in diameter. As there was no advance, I was notified and saw the patient at 6.45 P.M.

On external examination the fundus was 4 cm. below the ensiform cartilage. The uterus was almost tetanically contracted, so that accurate palpation was impossible, but it apparently contained a large child, whose heart sounds were most clearly heard to the right of the umbilicus. The scar

of the previous operation was not retracted, but palpation showed that dense adhesions existed between its lower end and the uterus. On vaginal examination the cervical canal was found to be obliterated, and the external os 4 cm. in diameter; its posterior lip was thin and on the level of the upper margin of the fifth lumbar vertebra, while its anterior lip was bulky and flabby and directed toward the junction of the first and second sacral vertebræ. The tissue in front of the cervix was very redundant and almost completely filled out the anterior portion of the superior strait. On introducing a finger into the cervix it was found that its anterior lip could be readily drawn forward, so it was thought that dilatation could be effected without difficulty and that the child, which lay in R. O. P. with its head freely movable above the superior strait, could be readily delivered by version and extraction.

*Operation.* The patient was anesthetized with chloroform and prepared for operation in the usual manner. The cervix was then readily dilated by Harris' method, but entirely at the expense of its posterior portion, as the anterior lip retained its original thick and flabby consistency. The right hand was then introduced into the uterus, but it was impossible to find a foot in spite of the fact that the tetanic contraction had, in great part, subsided under the influence of the anesthetic. During these manipulations the head became rotated so that it lay in R. O. P.; accordingly the left hand was introduced into the uterus, but with the same result.

This was very surprising, as there was every promise of the version being easy, and I could not account for my inability to find a foot. In order to ascertain the cause the right hand was once more introduced, when all the portions of the child except the feet could readily be palpated. Finally the legs were traced down as far as the knees, which disappeared in front of a peculiar, resistent, cresentic ridge of tissue which extended transversely across the uterine cavity about 10 cm. above the external os. This ridge measured about 2 cm. at

its thickest part, and gradually faded away toward the sides of the uterus. As the tissue below it was continuous with the cervical canal, it was at first thought that the structure represented an abnormal contraction ring, but on further examination this idea had to be abandoned, as a deep pocket lay in front of it into which the bent finger-tips could be introduced, though its base could not be reached. The feet could readily be palpated in this pocket, but could not be seized or dislodged in any way, owing to the constrained position of the hand.

Owing to the peculiar condition of the uterus and my inability to perform version, I asked Dr. Goldsborough to examine the patient and see what he could do. He confirmed my findings, but was likewise unable to seize a foot. I then attempted to adjust high forceps, and had an assistant push the head down into the superior strait as far as possible, but was unable to adjust it satisfactorily, partly owing to the high position of the head, but more particularly on account of the mass of tissue which filled out the anterior portion of the pelvic canal.

Notwithstanding the repeated attempts at delivery, the patient was in excellent condition and the fetal heart sounds unimpaired. Accordingly, it was determined to perform Cesarean section, and afterward to remove the uterus in order to prevent the possibility of infection consequent upon the repeated manipulations to which the patient had been subjected. This appeared to be the most conservative method of delivery, as it seemed very likely, even if we were willing to sacrifice the child, that its delivery after mutilation might prove as difficult as in Case I.

The patient was kept on the table under light anesthesia while the usual preparations for operation were being made. On opening the abdomen by an incision 15 to 16 cm. long it was found that the enlargement of the uterus had been effected almost entirely at the expense of the posterior wall and that the fundus lay only a few centimeters above the

symphysis. Extending from just below the fundus to the anterior abdominal wall, about 2 cm. above the symphysis, was a short band of dense tissue which apparently held the uterus in its abnormal position. Terminating at the adhesions was a narrow tongue-shaped process of omentum, which covered the portion of the uterus beneath the incision. This was ligated and cut at its distal end, and then the fixation band was tied with two ligatures and divided, but even then the uterus did not change its position.

A long incision was then made in the posterior wall of the uterus, and the placenta which lay beneath it rapidly pushed away, the membranes ruptured, a foot seized, and the child rapidly extracted. Following this the uterus was delivered and, after clamping the abdominal walls, amputated through the upper part of the cervix, the left ovary and tube being left *in situ*. The stump was then covered with peritoneum and the abdominal wall closed in three layers in the usual manner.

The patient made an uneventful recovery, the highest temperature being 100.7°, and left the hospital twenty-five days after the operation. The child was a boy weighing 4225 grams; it did extremely well and at discharge weighed 320 grams more than at birth.

On examining the amputated uterus, it was found that the fixation had occurred just between the insertions of the round ligaments, and, consequently, that practically the entire gestation sac must have been formed by its posterior wall. No trace could be found of the cresentic ridge which had been felt during labor. Notwithstanding the fact that the incision had been made entirely in the posterior wall, examination showed that both the anterior and posterior walls were of equal length and, therefore, must have hypertrophied to about the same extent. Accordingly, the condition noted at the time of labor could be explained only by supposing that the hypertrophied wall, instead of forming a thick mass of tissue, as so often occurs, was obliged to buckle or become

folded upon itself in order to accommodate itself to the space available between the adhesion and the cervix. If this were the case, the cresentic ridge felt during labor was nothing but the contracted top of the fold, and the difficulty encountered during delivery was due to the fact that the feet could not be dislodged from the sacculation in front of it.

The correctness of this supposition is demonstrated by the fact that identical conditions could be produced in the laboratory.

In my hospital and private practice I have notes of about sixty women who were delivered after ventral suspension or fixation, but serious difficulty was not experienced except in the four cases just mentioned. In these, however, the dystocia was most marked and necessitated the following operations: Two Cesarean sections, one most difficult craniotomy, and one fairly easy version and extraction, thus clearly demonstrating that while the course of labor following the operations in question is usually normal, it may occasionally become very complicated and demand the performance of most serious operations for its termination.

In each of my four abnormal cases, marked backward and upward dislocation of the cervix was noted, while the enlargement of the uterus occurred almost entirely at the expense of its posterior wall. On the other hand, the behavior of the anterior wall was not so uniform. Thus in the patient upon whom Lynch performed Cesarean section, it was practically normal; in the cases requiring version and craniotomy, respectively, it formed a thick pad of muscular tissue which encrouched upon and obstructed the superior strait, while in the last case it was folded upon itself as has been described.

Both of these former conditions are well known and have been noted by many writers, while that observed in Case II was very unusual and, as far as I can learn, has not heretofore been fully described. In this instance the anterior and posterior walls had apparently hypertrophied

to an equal extent, so that the former, in order to accommodate itself to the contracted space between the point of fixation and the cervix, was obliged to buckle and fold upon itself, thus forming a transverse ridge which practically divided the uterine cavity into two portions—the main cavity and a sacculation in front of the fold.

A somewhat similar observation was noted by G. Martin a few months ago, but he was inclined to regard it as due to an abnormal condition of the contraction ring. Likewise, it is possible that Noble had to deal with a similar condition in the case of Porro-Cesarean section which he performed in 1896, although in that instance he described the obstacle to version as a mass of muscular tissue instead of a fold or ridge as in my case. However that may have been, it is perfectly clear that the behavior of the anterior uterine wall is not identical in all cases, and it must be admitted that it may assume any one of the three conditions to which I have referred.

If my experience with dystocia following ventral suspension or fixation of the uterus is usual, there can be no doubt as to the propriety of absolutely condemning the employment of either operation in women during the childbearing period; whereas, if it is only exceptionally observed, such a conclusion does not necessarily follow.

A casual survey of the literature would apparently lead one to believe that my experience was very exceptional, particularly if one confines one's reading to the articles published in support of any one of the modern methods of suspension; although, of course, there is a general consensus of opinion that fixation should not be employed as long as there is any possibility of the occurrence of pregnancy. Thus, Holden, in a report upon the results following 900 ventral suspensions performed at the Johns Hopkins Hospital by Kelly's method, states that no serious consequences were observed in eighty-three pregnancies following the operation. Beyea likewise holds that the course of labor was practically normal in forty-

three deliveries following his method of suspension, in which the sutures were introduced into the posterior wall of the uterus just below the intertubal line. Moreover, Seegert, after studying the question of dystocia following this class of operations, contends that it can be avoided only by carefully following Olshausen's technique, that is, by employing the uterine ends of the round and broad ligaments as the points of fixation.

A somewhat similar view is expressed by Jacobs, who holds that dystocia is prone to follow all varieties of suspension except that devised by himself, in which, after a vaginal incision, the peritoneum just above the bladder is sutured to the anterior wall of the uterus.

On the other hand, when the literature is sifted from an obstetrical point of view, a totally different impression is gained, and one cannot avoid the conclusion that all of the operations thus far proposed may occasionally lead to serious dystocia.

Thus, Noble, in 1896, collected from the literature 206 cases of pregnancy occurring in women who had been subjected to ventral suspension or fixation, to which Andrews, in 1905, added 189 more, thus making a total of 395 pregnancies following such operations. In this series, 36 cases ended in abortion and 9 in premature labor; while in 42 out of the 350 cases which went to term, the following operative procedures were required to effect delivery: 21 forceps, 20 Cesarean sections, and 1 perforation. Moreover, the uterus ruptured in 3 other instances, while transverse presentations were noted in 10 cases.

What do these results indicate? This may be best answered by considering the several conditions separately. The occurrence of 36 abortions and 9 premature labors in the series does not necessarily indicate that they should be ascribed to the operations. This represents an incidence of 9 per cent. as far as abortions are concerned, which is no greater, and probably less, than would be noted in a series of pre-

sumably normal pregnant women. Indeed, my experience leads me to believe that the operations in question may even exert a favorable influence in this regard, as it not infrequently happens that women who have had a series of abortions before the operation, go on to term after it. Moreover, it must be admitted that many patients who have been sterile for years, promptly conceive and go through a normal pregnancy after the operation.

Likewise the necessity for 21 forceps operations in the 350 cases which went to term, an incidence of 6 per per cent., does not necessarily condemn the operation, as it is scarcely greater than will be noted in a series of ordinary labor cases. Nevertheless, a careful study of the reported cases would indicate, in some instances at least, that the necessity for the employment of forceps must be sought in dystocia attributable to the operation in question, and occasionally, as in the cases reported by Engström, the attempt at delivery by that means was ill-judged and proved to be more serious than Cesarean section.

On the other hand, the fact that Cesarean section was necessary in 6 per cent. and that transverse presentations occurred in 3 per cent. of the full-term cases, affords a weighty argument against the propriety of performing either ventral suspension or fixation in women during the childbearing period. The force of this argument is increased when it is remembered that in an active general obstetrical service Cesarean section is performed only about once in 200 cases, and transverse presentations are observed about once in 250 cases, which indicates that the incidence of the two conditions has become increased twelvefold and eightfold, respectively. Moreover, when one recalls the fact that rupture of the uterus was observed in 3 other cases of the series, it seems impossible to avoid the conclusion that the unusual incidence of serious dystocia must be attributed to the operations in question.

This disastrous effect upon the course of labor was clearly

illustrated by the experience of Maly, who reported the obstetrical history of a woman who had nine children, and whose uterus was suspended after the fifth pregnancy. Prior to the operation all of her labors had been spontaneous, but in each of the four labors following it, difficult version and extraction were necessary.

The question of Cesarean section following these operations was studied in detail by Lynch, in 1904, who collected 21 cases from the literature, with 8 deaths. In the following year, additional statistical studies were made by Andrews and Seegert, who increased the number of Cesarean sections to 31. Since then I have collected 4 others, namely, those reported by Clark and Bowley, G. Martin, Boyd, and Smith, so that, including the case here reported, one finds in the literature 36 cases of Cesarean sections following these operations. Moreover, when one adds to these the 3 cases of rupture of the uterus, as well as the 3 cases of craniotomy reported by Kober, Staude, and myself, it is apparent that further argument is unnecessary.

What is the mode of production of the dystocia? The first essential is the fixation of the uterus, and the firmer it is and the further back upon the posterior wall of the uterus it is situated, the more serious will the obstacle to labor prove to be. Whenever the fixation is firm, the normal expansion of the uterus must be interfered with, as its anterior wall cannot rise out of the pelvis, and consequently the enlargement of the organ is effected almost entirely at the expense of its posterior wall, which soon becomes unusually thin and at the same time exerts excessive traction upon the cervix, drawing it far posteriorly, and occasionally actually up into the abdominal cavity. At the same time the anterior wall of the uterus usually undergoes characteristic changes, as it hypertrophies and becomes converted into a thick, muscular pad, which encroaches upon the superior strait, or, occasionally, as in the case here reported, becomes buckled upon itself and leads to sacculation of the uterus.

Unfortunately the position of the cervix, during the last weeks of pregnancy, does not afford a satisfactory means of predicting the course of labor. In some cases its dislocation during pregnancy may not be so pronounced as to lead one to fear serious trouble, yet at the time of labor it may become seriously dislocated. This is due to the fact that the contractions of the anterior wall of the uterus play little or no part in its dilatation which is effected almost entirely by traction exerted by the muscular fibers of the posterior wall. Consequently, dilatation occurs only in the posterior segment of the cervix which gradually becomes drawn up as the labor advances, so that in extreme cases its upper margin may eventually come to lie as high as the level of the second and third lumbar vertebræ.

Moreover, the dislocation of the cervix seriously interferes with its dilatation, for instead of the internal os lying in the axis of the uterus where it can be subjected to the normal dilating action of the bag of waters, it may be so displaced that this cannot occur, and the hydrostatic pressure will be wasted in the futile attempt to dilate a portion of the anterior wall where no opening exists. Consequently, the dilatation is delayed or may not occur at all, and it may happen that rupture of the thinned-out posterior wall may occur while the cervix is but slightly dilated.

Moreover, the hypertrophied anterior wall may interfere with the engagement of the presenting part, and thus favor the production of transverse presentations; and even when the dislocation of the cervix is not extreme, it may seriously interfere with the introduction of the hand into the uterus in the various procedures undertaken to effect delivery of the child when Cesarean section does not appear to be indicated.

Naturally the advocates of the various methods of ventral suspension are perfectly willing to admit that all of the evils mentioned are likely to follow firm fixation of the uterus to the anterior abdominal wall, and are prepared to insist that

the latter operation is inadmissable in women during the childbearing period. But in making this admission they will contend that it does not hold good for the particular method of suspension which they employ, which they hold is devoid of danger when properly performed. At one time I held similar views and believed that Kelly's operation could not be followed by dystocia if done according to his directions.

The four cases of dystocia, which I have observed, however, prove the fallacy of such reasoning. In one of my cases an intentional fixation had been done by an unknown operator, but in the other three typical suspensions had been performed. Moreover, it cannot be contended that the latter were improperly performed as all of them had been done by most trusted assistants, of Dr. Kelly, who were thoroughly well acquainted with the most approved technique.

As the result of my experience and a study of the literature, it would seem that in the vast majority of cases ventral suspension is devoid of danger as far as childbearing is concerned; but occasionally, even in the hands of the most expert operators, as the result of some error in technique or from some inexplicable cause, the proposed suspension fails to occur and the uterus becomes firmly fixed to the abdominal wall, and then becomes a menace during pregnancy and labor. As such accidents may happen to anyone, I feel that ventral suspension may become a serious source of danger, and, therefore, it will have to follow the fate of ventral and vaginal fixation, unless all possibility of the occurrence of pregnancy has been removed. Accordingly, I have abandoned its employment and at present I am experimenting with Gilliam's operation, but I am constantly bearing in mind the possibility that it may likewise share the same fate.

## DISCUSSION ON PAPERS OF DRS. MORAN AND WILLIAMS.

Dr. Robert S. Hill, of Montgomery, Ala.—I am very much gratified to hear the views expressed by Dr. Williams. In considering any abnormal condition we naturally turn to the cause, and, therefore, a discussion of dystocia following ventro-attachment involves a discussion of ventrofixation and ventrosuspension. It has been my opinion for some years that these operations should not be done when the uterus was active, and I have taken advantage of several opportunities, one in Birmingham at the meeting referred to by Dr. Williams, to raise my voice in opposition to them. When we consider the marvellous growth of the uterus under the stimulation of pregnancy, we can not rid ourselves of the opinion that one of four things must happen, viz., the attachment give way, the band or bands stretch beyond recovery, the organ prematurely empty itself, or dystocia result. I care not what operation is performed nor how well it holds an organ in its normal position, if it tends to interfere with the functional activity of the organ it is an illogical procedure. We are prepared to assent to the proposition that there was a time when ventrosuspension and ventrofixation formed the lesser of two evils, retrodisplacement being the greater; but, in my opinion, these operations have served their purpose in the evolution of the treatment of retrodisplacement of the active uterus, and must now be discarded for procedures that are directed to the restoration of the normal supports of the organ. We must address ourselves to the shortening of the ligaments of the uterus, and when we can successfully do this, as I believe we now can, we need have no fear of dystocia resulting from our efforts.

Dr. Henry O. Marcy, of Boston.—I wish to express myself as being opposed to anterior fixation of the body of the uterus, although I have done it many times and have studied very carefully the results. As a consequence, I adopted some years ago the fore-shortening of the round ligaments from within, by intrafolding, so that the same attachment is made on either side.

In his paper, Dr. Williams, with his experience, has taught us a new phase of the subject. I concur in the remarks of the last speaker, that it is important we do not interfere with the body of the uterus in our fixation operations, and it is easier to make

anterior suspension, as it has been called, in a bilateral way, operating from within upon the round ligaments.

The so-called Alexander operation, by the general consensus of opinion, has long since ceased to be advised. There are two or three reasons for abandoning it. First of all, we do not know the exact peri-uterine conditions. We cannot tell the type of the pathological conditions which may be within the abdominal cavity; therefore, without going into a detailed argument, I will simply say that we should open the abdomen, make a bilateral suspension in a pathological way, with the expectation of a physiological result if pregnancy should follow.

Dr. Charles P. Noble, of Philadelphia.—I have studied this subject carefully and statistically. At that time the question of how the operation was done was not well worked out, and the distinction between suspension and fixation was not clear. It was thought at the time that the difficulties all came from the fixation of the uterus instead of its suspension. Since that time such evidence, as has been presented today by Dr. Williams, has been accumulated, so that everyone now recognizes that when the uterus is attached to the abdominal wall we run the risk of fixation with its accompanying troubles.

I wish to say a few words on the obstetrical side of the subject. I do not see as much obstetrics as I used to; but several of these cases have come under my care, particularly illustrating the condition which Dr. Williams referred to. This condition was present in a woman upon whom I performed a Porro operation, and which was reported in the paper I referred to. I believe that in the late cases, not the cases that are properly conducted but in the class of cases which one will see in emergency hospitals where patients are already infected, there is still an indication for the old-fashioned Porro operation with the extraperitoneal treatment of the stump. If I had to deal with such a case, with a dead fetus, and the woman infected, I would not open the uterus until the abdominal cavity had been closed, but treat it with serra neud or elastic ligature. That is the only point which I feel is of interest with reference to the obstetrical aspect of these cases.

I would like to say a few words about how they shall be dealt with primarily. All of us have felt more and more inclined to get away from suspension operations. I have done them, but I am constantly doing other operations, more and more.

So far as the Alexander operation is concerned, in spite of what

the last speaker has said (Dr. Marcy), it is an ideal operation when there is no trouble within the peritoneal cavity, and having had experience with between one hundred and fifty and two hundred such operations, extending over fourteen years, I have no reason whatever to change the opinion I have held regarding it. I like the operation far better than I did ten years ago. So far as the patients themselves are concerned, I have no more grateful group of patients than those who have undergone the Alexander operation, so that in its proper field it is an ideal operation, that is, for virgins, for women who have not had gonorrhea, and women who have not had sepsis in childbed. In the class of cases in which it has a field, and there is nothing wrong with the appendages, I think it is one of the best operations we have in gynecology.

What shall we do with those cases that have disease of the appendages, where we have to open the abdomen? The Gilliam operation never appealed to me personally, because we draw the round ligaments across the peritoneal cavity and divide the entrance into the pelvis in three segments; and it has been surprising to me that we have not heard of more cases of obstruction of the bowels following adhesions around the round ligaments. I have never done the operation, therefore, I am in the illogical position of being opposed to it without having had any experience with it. However, some years ago, when Simpson, of Pittsburg, improved on the work of Ferguson and Gilliam, advising the retroperitoneal shortening of the round ligament, thus obviating the objections to the Gilliam operation, I began doing that operation, and have been doing it more and more. For some little time I have taken advantage of the one step of the Gilliam operation, as modified by Dr. Ill, and tacked it on to what Simpson had proposed, and in this way doing retroperitoneal shortening of the round ligaments as a routine operation when the abdomen has to be opened.

So far as the immediate results are concerned, it leaves nothing to be desired. Theoretically, it ought to do well in cases of pregnancy. My own feeling about the matter is that at present neither the Gilliam operation, the Simpson, nor the Ferguson has been sufficiently tested by time and experience for us to speak with authority. I believe we are in the experimental stage, with the intraperitoneal operation of shortening the round ligaments, just as we were in the past with reference to suspension operations, and, personally, I am not prepared to believe that we know all about the question at the present time.

Dr. Robert T. Morris, of New York.—We must address ourselves to the ligaments. I cannot do the Alexander operation because I put too many old adhesions on the stretch, and the operations which depend upon adhesions for suspension are not satisfactory because they give way. One course I have employed in a great many cases is, very briefly, this: I draw the round ligament up in this way (indicating) with a hook, I shorten it up; I put three inches of the ligament out of commission; then suture it with kangaroo tendon in this way (indicating), leaving a good deal of the round ligament. This is drawn out from the broad ligament in such a way that I have the muscular and fibrous structures of the round ligament brought into apposition. This gives me firm union and an elastic support when the uterus is held up in this position. If I tip the uterus back forcibly into position for a moment, for six or eight inspirations, the uterus is back where I want it. I do not know of any cases where I have had dystocia after this operation. It is true it may be like the cases that were described in one of our foreign medical journals recently, where an author described a method of curing syphilis by injections of mercuric salt beneath the skin. The patients all had abscesses, often painful abscesses, after the injections, but he argued he cured the patients because they did not come back for further treatment. I may have had cases of dystocia, but never have heard of them. (Laughter.)

Dr. Edward McGuire, of Richmond, Va.—I want to say a few words in regard to these conditions. I think all of the operations for posterior displacement are makeshifts, and most of them are unsatisfactory. I agree with Dr. Noble that in uncomplicated cases the Alexander operation is the one to be preferred. The suspension operation which I prefer is the one suggested by Dr. Charles Mayo, which consists in passing a pair of forceps between the fascia and the muscle to the internal abdominal ring, then through the ring subperitoneally along the ligament to within an inch and a half of the uterus. The ligament is then caught and pulled through and fastened into the abdominal incision to the muscle. I prefer silk sutures as a means of fastening the ligaments as I do in all cases of suspension of the uterus by the ligaments.

In the Gilliam operation I have seen this ligament stretch to the extent of six inches, and in ordinary cases of suspensori uteri, after Kelly's method, I have seen the ligament elongate sufficiently to allow the fundus to fall into the cul-de-sac, in one instance

eight inches in length. Indeed, I have seen failures in operations for posterior displacement of the uterus except from Alexander's and the Mayo operations. The latter operation I have not used long enough to observe, so far, any failures.

Another thing, it is well known that in all cases of posterior displacements of the uterus, in the first stage of it, there is descent of the uterus. The uterus is below the normal level in the pelvis. If you can raise the uterus to its proper level in the pelvis and keep the cervix within an inch of the hollow of the sacrum, the fundus will take care of itself. The reason why the ligaments all stretch in the Gilliam operation is because the uterus has a tendency to drag down—to pull down the ligaments. I have, in those cases where there is a marked descent of the uterus, adopted the method of making an incision around the cervix and attempt to shorten the uterosacral ligaments in the same manner as is done for prolapse of the uterus after Dudley's method. That lifts the uterus up, and, at the same time, fixes the cervix. That method is useful in all cases where there is marked descent of the uterus along with the posterior displacement. My experience has been, however, that all suspension operations have a certain percentage of failures. I have personal knowledge of 28 cases of pregnancy following this operation of suspension of the uterus. In 1 of the cases the uterus was high up and the cervix pointed backward. The pressure of the child was on the anterior surface of the uterus.

Dr. Joseph Taber Johnson, of Washington, D. C.—First, I want to say a word or two with reference to Dr. Moran's paper. It seems to me he has given a method which means much to the obstetrician. I have seen cases of delivery where there was cancer of the neck of the uterus, or where there was eclampsia; the difficulties we had in the safe delivery of those women might have been overcome by the method proposed by Dr. Moran. In cases where puerperal convulsions have been going on for some time; where there is a long and undilated os; where the Bossi and other dilators would take half an hour or longer to dilate the uterus, and where interference by manipulations, etc., would keep bringing on renewed convulsions, it seems to me that if vaginal Cesarean section can be safely performed in a few minutes and relieve these women of their convulsions, a great improvement in obstetric methods has been made. It can be done by a man who is not so very skilled in surgery. The operative technique seems simple. A point that struck me is that these patients can be

operated on safely in their own homes without taking them to hospitals, provided the surroundings can be made aseptic, without adding much risk to the patient or shock to the members of the family, who often strenuously object to the removal to hospitals during the labor—when the loved one is dangerously ill.

I want to say a few words in regard to Dr. Williams' paper. In a paper read by me at the Birmingham meeting of this Association, I tried to sustain the argument that suspension of the uterus did not interfere so largely as had been claimed with pregnancy and labor. The statistics which I relied on mostly at that time were given to us by Dr. Noble, in a paper published by him a few years ago, in which he went largely into the statistics of these cases, and reported several hundred—yes, I believe, several thousand—cases he had collected from different operators in this country and abroad, and his final conclusion was that only about 1 in 40 cases subjected to this operation had any dystocia afterward. He argued, and I also did in my paper, that we had trouble in about 1 in 40 cases of ordinary labor, and because 1 in 40 women subjected to this operation had some form of dystocia this fact did not prove the trouble was due to the operation. But as our evidence increases, and as Dr. Noble has receded from the position he took at that time, we must necessarily be more or less influenced by the additional evidence we have heard today as to the effect of this operation on women who are likely to bear more children. I still maintain, however, that the suspension operation is not necessarily a dangerous one when it is properly performed upon the few women who subsequently become pregnant.

The closing argument of Dr. Williams was a sound one from his standpoint. The operations in the cases Dr. Williams has reported were done by Dr. Kelly's assistants, who were taught by Dr. Kelly; therefore. the presumption is they were done perfectly. Suppose all agree that *fixation* of the uterus ought never to be done on women who are likely to have children; some operations, however, begun as suspensions have unintentionally ended as fixations. If a suspension of the uterus is undertaken, and fixation later occurs in a few cases, it strikes me that until all these gentlemen have settled on some particular operation, upon which half a dozen of them can agree, that it is *the* operation to do, instead of the one of sixty or more varieties of operations on which no two of them have agreed. Suspension of the uterus, properly done, *without fixation,* is an operation which

presents more advantage to a woman than some of the varieties which are still in their initial stages, and which have not yet been employed long enough to have the final reports come in in their favor or against them. I do not think it is at all settled that any one of these operations is the operation which the whole profession can agree on as *the* best operation for the future. Therefore, a suspension operation on women not likely to have children, or even if they are likely to have children, is not contra-indicated, because there is no proof that all of these substitutes for suspension will be absolutely free from complications or from dystocia during subsequent pregnancy and labor. Dr. Noble said he had no more grateful patients than those upon whom he had done the Alexander operation. I may say that I have never had more grateful patients than those to whom I have afforded relief by a suspension operation. Everybody who attempts to do these substitute operations cannot always be successful. They are not indicated to be done when there is disease of the ovaries and tubes, and when there are adhesions, as so frequently exists.

In the great majority of cases where the operation is indicated, on account of pain in the pelvis and a number of other symptoms, there is generally associated, with the retroverted condition of the uterus, some primary cause which has produced the retroversion, which is not curable by an Alexander operation and can only be cured by such an operation as will enable the surgeon to deal with the associated and primary pathological conditions inside of the abdomen. Therefore, I say, the Alexander operation has a limited field because the conditions which have produced the retroversion are not touched by that operation, and cannot be, when the uterus is bound down, and where associated tumors, inflammatory conditions, pus tubes, ovarian abscesses, or something of that kind exists, and which constitutes the chief part of the symptoms of the sufferer.

Dr. J. Wesley Long, of Greensboro, N. C.—I am going to do an almost unwarranted thing—propose an operation I never did, never saw done, nor never heard suggested; but I am going to mention it as a part of the history of the evolution of this subject.

I have been doing the suspensio-uteri operation ever since I saw Dr. Kelly do it some fourteen years ago. While I have had some cases of pregnancy following that operation and no dystocias, yet for a number of years I have been convinced it was not a scientific operation, not surgically correct, so I have abandoned the operation. Since a year ago, when I saw Dr. C. H. Mayo

do his operation for shortening the round ligaments, with which you are all familiar, I have employed that method in 25 or 30 cases. It is a most satisfactory operation, whenever it is necessary to open the abdomen.

I agree thoroughly with Dr. Noble that the Alexander operation is the ideal operation when it is unnecessary to open the abdomen, and I practise that method, in cases of that kind, with the most satisfactory results.

In studying over this matter I have become convinced that even the Mayo operation does not quite meet the requirements or the indications. Of course, I am referring now to cases in which it is necessary to open the abdomen.

Two considerations should be borne in mind: first, all operations for shortening the round ligaments should be done on the distal ends of the ligaments, the most fragile and weakest portion; second, no operation on the round ligaments done inside of the peritoneal cavity is satisfactory for many reasons. Therefore, the operation which I propose is to shorten the round ligaments preperitoneally. This is easily done.

The last operation I did for retrodisplacement, a few days ago, after dealing with the lesions found in the abdomen, I stripped the peritoneum from the incision down on either side to the round ligament. Catching a loop of the ligament I pulled it up, but I had not worked out in my mind the best way to deal with it; therefore, in that instance, I finished the operation after the Mayo method, *except that the forceps, after going through the fascia and muscles, caught the round ligament preperitoneally and not intraperitoneally, as is done by Mayo.*

One or two anatomical points should be remembered. You will recall that when the round ligament leaves the uterus it passes outward to the pelvic wall between the folds of the broad ligament, then forward and upward. As it bends forward and upward it passes on the outer side of the remains of the hypogastric artery. If you catch the ligament distal to this point and make traction, you will find that where it curves around the hypogastric artery there is a kind of natural pulley. The operation that I propose and have partially done preserves this normal relation; besides, it leaves no intraperitoneal loops, stitches, nor other points about which an adhesion may form.

Dr. W. M. Jordan, of Birmingham, Ala.—Dr. Johnson remarked that it is difficult for any half-dozen men to agree on any one operation which will relieve a retrodisplaced uterus. There is

one operation which will relieve every case and following which there is no liability for dystocia, and that is hysterectomy. I think that operation ought to be done a little more frequently, especially for flexions, and palliative operations a little less.

Dr. Williams (closing).—My object in presenting this paper and in reporting these cases was not to say that suspension of the uterus is always a bad operation. It is frequently a good one. But the point I want to make is, no matter how carefully we do it, we may unwittingly convert a suspension into a fixation, and if we have got fixation we have a condition for a serious dystocia. If we could be sure in every case, that we can produce the desired suspension, then it is a matter we should talk about. But we are unable to assure ourselves of it, and as long as we are likely to have this thing turn up at once in 100 cases, or may be oftener, we are not justified in my judgment in subjecting a woman to one risk in a hundred or 50 Cesarean sections if we can avoid it. I think vaginal Cesarean section has a great field and is a most useful operation.

There is one point on which I would like to take issue with Dr. Johnson, and that is this: it is not an operation to be done by the ordinary practitioner, and it is not an operation to be done in the ordinary house. It is an operation that requires a man who knows a good deal about operating, to do it; it requires skilled assistants. I would not advocate the operation without the assistance of two or three good men, to one of whom should be assigned the administration of the anesthetic. In short, I think the operation should be limited to those men who have hospital facilities and skilled assistants.

Dr. Stone.—To develop this matter, I will ask you, Dr. Williams, this question: If you were away from hospital facilities and had a case of eclampsia, and time was precious and there was no time to remove the patient to a hospital, and you had to do some operation, which would you do: try to dilate and deliver, or resort to vaginal Cesarean section?

Dr. Williams.—If I could not get trained assistants, I would not do vaginal Cesarean section. If she had a rigid cervix I would hesitate a long time about dilating manually or with instruments, because then we might get a deep tear of the broad ligament and have the woman bleed to death on the table, or it may give rise to other serious difficulties. The man who is not equipped to do the proper sort of work in this class of cases should restrict himself to medical means, and let the woman die from eclampsia rather than feel that he killed her by some ill-chosen operation.

# A BRIEF SKETCH OF ONE OF BALTIMORE'S GREATEST MEN, HORATIO GATES JAMESON, M.D.

## HIS MOST VALUABLE CONTRIBUTION TO SURGERY: THE OCCLUSION OF ARTERIES BY THE BURIED ANIMAL LIGATURE.

By Henry O. Marcy, M.D.,
*Boston.*

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The small army of investigators today are so intent upon their studies and the profession at large so interested in their results, that most of us forget the painstaking research work performed by equally acute observers in the past.

Many of the more valued observations of the present were carefully outlined, and in a measure executed, at periods so long ago that they have now been forgotten. It remains for the historian in medicine to properly make record of the splendid achievements of the past by men whose names are unknown to the present generation. I have often wondered that this fruitful field has been so sadly neglected; quite as much to the detriment of the present as to the lack of respect which is due our predecessors. Owing to the brilliant triumphs of science, during the last generation, medical research work was greatly neglected until the recent past and now we bespeak for it, in the light of new investigations, an interest and profit equal to, if not surpassing, that of surgery. I will be pardoned a single illustration. When a student in Berlin, in the early seventies, I became greatly interested in the studies of Wounderlitch upon fevers.

I brought with me, upon my return to Boston, a number of clinical thermometers which I gave to medical friends. So far as I then knew they were the first used in America.

It may be known to some of you that Dr. Currie,[1] in 1797, invented clinical thermometers, even with registers, which for a considerable time were used in England.

Recently, I chanced to find an essay which was awarded a prize, in 1819, by the Boylston Medical Society of Boston, written by Dr. Enoch Hale, Jr., member of the Massachusetts Medical Society, in which he records a series of very careful observations to prove the absorption of drugs by intravenous injection. Boylston Prize Essay, 1821—"Dissertation on the Propriety of Injecting Medicinal Substances into the Veins." The interesting thing here to note is his careful observation of temperature taken under the tongue by the use of a clinical thermometer.

To our beloved Lord Lister, of England, is generally accorded the honor of introducing to the surgical profession the use of the animal ligature for the occlusion of arteries. His research work in this direction is more or less familiar to every student of modern surgery. The value of his contributions to surgery can hardly be overestimated. They are fundamental and revolutionary, and every practitioner of surgery in the civilized world is under a daily debt of gratitude to this most distinguished of living surgeons. But for this basic knowledge of bacterial infections, and how to avoid them, his studies upon the occlusion of arteries would necessarily be of comparatively minor value.

Even Mr. Lister, a man of wide erudition and research, was entirely ignorant of the work of a predecessor in this field, to whom now I have the honor of calling your attention. This was the more natural, since I suppose today our English friends will admit that they earlier shared with Sidney Smith in the feeling which he expressed when he satirically asked, "Who reads an American book?"

[1] James Currie, born 1756, died 1806.

In this goodly city of Baltimore, in the early period, there lived a surgeon who is entitled to monumental distinction and enduring fame, Dr. Horatio Gates Jameson. He was a surgeon of distinction, although, of course, a general practitioner of medicine. His publications were not numerous, as adjudged by a modern standard, but he wrote an admirable treatise upon fevers, a book upon *Domestic Medicine,* and was the editor of a medical journal for years. His conclusions were quite at variance with the accepted teachings of the day, but re-read in the light of our present knowledge would be considered fundamental and orthodox.

The sources of information, of a man so famous, are extremely meagre, although men still living have him in memory. Our late friend, Dr. Gross, of Philadelphia, was for a time his junior assistant, and Dr. Gouley, of New York, has recently written me, in an interesting way, of his personal remembrances of Dr. Jameson.

One of the more recent and most interesting of medical publications is entitled *The Medical Annals of Maryland,* by Dr. Eugene F. Cordell, of Baltimore. I have availed myself of its pages in the preparation of this article.

Few men did so much to give credit to early American surgery as Dr. Horatio Gates Jameson, who from 1820 to 1840 carried the fame of Baltimore, as a seat of medical and surgical research, to all parts of the civilized world. This truly great surgeon was a native of York, Pa., where he was born, in the year 1778. His father, David Jameson, also a physician, had a most interesting and adventurous career. A native of Scotland and graduate as Doctor of Medicine at the University of Edinburgh, he emigrated to Charleston, S. C., in 1740. He was accompanied on the voyage by Dr. Hugh Mercer, who settled at Fredericksburg, Va., and became a Brigadier-General in the Continental army, losing his life, as is well known, at the battle of Princeton, in 1777. Dr. David Jameson also removed from Charles-

ton and settled at York, Pa.; he died at Shippensburg in the same State, about the close of the century. He was a Lieutenant-Colonel in the French and Indian war and was badly wounded during that period in an engagement with the Indians. During the revolution he was Colonel of the third battalion of York County Association. In 1842 the son gave a charming description of his home at York, in a Baltimore periodical; also an account of his father's voyage across the Atlantic, describing among its hardships the short allowance of musty bread and foul water and the rusty salt beef doled out, yet consumed by crew and passengers with avidity.

Dr. H. G. Jameson studied medicine under his father and began practice, in 1795, at the early age of seventeen.

During the war of 1812 he held the office of surgeon to the United States troops at Baltimore, for which service his widow received a pension after his death.

He was physician to the city jail for several years; from 1814 to 1835 he was surgeon to Baltimore Hospital, and from 1821 to 1835 he was consulting physician to the Board of Health.

In 1830, on special invitation, he visited Europe, sailing by the packet running from Baltimore to Germany, and read before the medical section of the Society of German Naturalists and Physicians, which met in September of that year at Hamburg, a paper on the "Non-contagiousness of Yellow Fever." He was the only American delegate on this occasion and the first American to attend these meetings and felt deeply the honor of his position. His paper was read in translation at the meeting and was published on his return, in his journal. During his brief stay of two months in Europe he travelled through various European countries, staying longest at Copenhagen, to the American representative at which city he bore despatches from our government at Washington.

In 1835, while on his return from Texas, where he had

purchased lands, he received and accepted an offer of a chair and presidency in the Ohio Medical College, at Cincinnati. His colleagues there were Drs. Gross, Drake, Rivers, and J. B. Rogers, the last his late colleague at Baltimore. He removed thither with his family in October, 1835, and lectured during the ensuing session.

During a visit to New York he was taken suddenly ill and died August 24, 1855, aged seventy-six years. His remains were brought back to Baltimore and interred in the Baltimore cemetery, corner of Gay Street and Boundary Avenue.

Dr. Jameson was about five feet ten inches in height, well built, erect, and muscular, but not corpulent; his head was covered with a sufficiency of snow-white hair; his face was always clean shaven, his complexion florid and healthy; his eyes dark brown and piercing and surmounted by bushy eyebrows; his face remarkably smooth and free from wrinkles. He retained his strength and power of endurance to the last. He wore heavy—remarkably heavy—gold spectacles. He dressed in black, wore a black tie, and was very careful and neat in his appearance—no one ever saw him look untidy. He spoke German, and was noted for his mechanical ingenuity.

Dr. Gross mentions him as particularly conspicuous in devising procedures for the radical cure of hernia. His operation of amputation of the cervix uteri for scirrhus, performed in May, 1824, has been referred to. It was the first done in Great Britain or America, but had been done in Europe by Oslander in 1801, and Lisfranc in 1816. It was Jameson's intention, in this case, to perform a vaginal hysterectomy. From the normal mobility of the uterus he believed he would be able to draw the uterus down low enough to enable him to pass a ligature behind it by means of a double cannula and wire. But he failed to get his toothed forceps into the uterine cavity, or to even get a curved needle armed with a ligature through the indurated cervix, and had to

be content with pulling upon the vagina with tenacula and thus bringing the os into view.

The object of my calling attention to his teaching at this time is to emphasize the value of a medical prize essay, entitled "Observations upon Traumatic Hemorrhage, Illustrated by Experiments upon Living Animals," *Medical Recorder*, Philadelphia, 1827, vol. xi. He takes for his text a quotation from John Bell, then perhaps the highest surgical authority:

"*Is not this fear of hemorrhage always uppermost in the minds of young surgeons? Were this one danger removed he would go forward in his profession almost without fear.*"

Dr. Jameson reviews, with much care, the teaching of the period and its great masters. He makes an especial emphasis upon the anatomical differences of the arteries, veins, and lymphatics, and shows that tissue arterial, when obliterated, is absorbed leaving *in situ* only connective-tissue cells. "Thus, when we tie an artery its contractile power is exerted in bringing the sides inward, which lessens the calibre, and, pressing the coats firmly against each other, there is considerable resistance to the deposition of lymph between them and the inner surface, a small distance from the ligature, as well as under it is brought into contact by the ligature, and thereby the fullest effect is given to the first effusion of lymph to produce a union of the walls; besides, we may reasonably suppose that the artery supplied with the blood, in its fullest formative powers, will heal better than if filled with blood, deteriorated, as it is in the veins. On the other hand, a vein having no contractile power lies flaccid when tied on that side of the ligature nearest to the heart, and becomes distended and large by the pressure of the blood on the other side of the ligature. Then the distention on the other side, by the coagulum of blood, prevents the approximation of the parietes, and the flaccidity of the coats on the other invites a free deposition of lymph between the coats, and thus does Nature, in her work, provide the means

of obliteration, notwithstanding the uncongenial properties of the blood within the vein. All this accords with our experiments upon the vessels in living animals, and we have found that if we merely interrupt the flow of blood through the artery, and irritate the coats so as to produce inflammation, we shall cause the artery to close more slowly and somewhat similar to what we see in the veins, by thickening of the coats, and obliteration without actual adhesion of the inner coats. All of which will be more fully illustrated in the detail of our experiments.

"If a patient be subjected to a great loss of blood at once, or by repeated smaller quantities, we shall have sooner or later an increased volume in the radial artery which would often deceive the most skilful, were he not apprised of the loss of blood. We have heard of such mistakes, and well remember a case where a boy, having wounded an artery in the plantar arch, bled violently from day to day, for about two weeks, notwithstanding a practitioner had used all the usual means of compression. When called to the case we took up the posterior tibial artery, and compression now succeeded in completely arresting the hemorrhage, but, although the patient had lost so much blood as to disable him for four days after the operation, to sit up at all, and although his face was as pale as death, his pulse was full and frequent; and though very compressible when closely attended to, yet, without the greatest attention, one would have supposed there was high excitement.

"That arteries will heal when the blood ceases to circulate through them is a thing universally known, and it is equally well known that this may be effected by compression in various forms—by plugging, tying a mass of fleshy structure around them, by diversion of the blood through other vessels as in the umbilical arteries and their immediate associates, and, also, the ductus arteriosus, etc. So innumerable are the instances of this kind that the opinion of Dr. Jones respecting the necessity of cutting the inner coats of arteries

with our ligatures is truly surprising, the more so, as it has obtained the very general approbation of the profession. Among those who have ably aided in refuting this visionary notion may be placed first: A. Burns, and C. Bell; Scarpa, Crampton, and Travers."

It is interesting, in our present knowledge of infections, to quote the following:

"Mr. Travers tells us that Mr. Coleman, a professor in the veterinarian schools in London, assured him that in the horse even small wounds of the skin would seldom heal by the first intention. Our experiments upon animals induce us to believe that the remark of Mr. Coleman will apply to all brute animals upon which we make experiments upon the bloodvessels. Few wounds heal well by the first intention and, indeed, it would seem to be a fair inference to make, *a priori*, that the structure of animals destined to live from one-fourth to one-half of the period allotted to man would be less perfect and less capable of bearing violence done to it than the more complete structure of man. The presumption then is, that, in a vast majority of cases, indeed almost without exception, if we tie an artery, the coats of which are healthy, in a proper manner, it will heal. It will follow then, as our experience has already gone far to prove, that secondary hemorrhage is, in ninety-nine cases in the hundred, owing to bad surgery."

The deduction which he makes upon the character of the ligature are of interest.

"The irritation of a rough ligature, continued a long time about the vessel, together with the attempts which are generally made by surgeons to get away the ligature by pulling, leads to suppuration and erosion of the coats of the vessel. We hesitate not to say that no such consequences will follow the use of the animal ligature, though we would disapprove of tying a nerve where it could be avoided; but we can easily imagine cases in which a surgeon, for want of assistance, might be compelled to dip down a suture needle and

enclose muscles, nerves, and everything around, with the bleeding vessel; and we venture to say that perfect safety will attend the use of the animal ligature under such circumstances."

A little prior to Dr. Jameson's studies, the eminent English surgeon, Mr. Lawrence, in the effort to lessen the "irritation" of ligatures upon arteries, brought out his, then, famous teaching that this was to be effected by using the minimum sizes of ligature material. He emphasized by asserting that the ligature best used in the occlusion of a medium-sized artery should not exceed, in weight, a sixtieth of a grain. In criticism of this Dr. Jameson observes:

"Upon the whole we think much credit is due Mr. Lawrance, and some others, for the introduction of the fine silk ligature into practice, since, before the use of the leather ligature, it was certainly a very great improvement; but, at the same time, we must declare, as our opinion, that this ligature was only suited to arteries of moderate size and that when you enlarge the ligature of silk to suit arteries of the first order, it possesses no advantage over the common-flax ligature, since both are alike indissoluble, and indestructible by the absorbents. Here, then, we see the superiority of the leather which, by the time it has answered the purpose of obliterating the vessel, is dissolved by the fluids and enters with them into the absorbing vessels of which we have had actual knowledge. . . . . We believe that the animal ligature will secure the patient from all these dangers, except one (to wit), the awkwardness of the surgeon; and even in this respect the animal ligature is preferable, not requiring that precision of management essential to the cutting ligature. If the ligature is cut from the leather with care it will always admit of being tied sufficiently tight, but can never be made to cut the coats, provided it is made of soft buckskin and not hardened by drawing it. It is less likely to slip when somewhat insecurely applied, because being elastic and soft it is spread over a small space of the

vessel, and almost immediately adheres by its glutinous qualities; it lies more securely, while the cutting ligature resting on a mere line, and having neither adhesive properties nor the advantage of a small vacuum between the vessel and the ligature, as is the case with the flattish adhesive ligature, is more likely to slip off. Besides, as we cut off the ends close, there is no risk of pulling them away by an "accidental jerk of the hand." In support of these assertions we have to offer the experience of several years' practice, during which we have used no other than the buckskin ligature, and no such thing as a secondary hemorrhage has ever occurred in our practice, extending alike to cases of aneurysm, tumors, and amputations. We are, moreover, decidedly of the opinion that in no case whatever have we had reason to suppose that the healing of a wound, accidental or surgical, was delayed by our ligatures: we never see anything of them after their application."

I quote the following clinical case with deductions by Dr. Jameson, since it shows clearly that he had considered the possibility of closing the wounds made in the veins by sutures for the purpose of preserving their calibre and continuity: "A healthy young man had been wounded in the thigh by a musket ball; at the end of three weeks the femoral artery sloughed, and the patient was only saved from imminent peril by the presence of a surgeon who instantly made pressure. Dr. ———— was called on and immediately proceeded to tie the artery above the profunda, not knowing at that time whether the profunda or femoral was the vessel injured. Two ligatures were applied and the artery divided between them, in doing which the surgeon, instead of putting his knife under and cutting upward, cut from above downward and cut the femoral vein more than half off. There was an alarming hemorrhage from the wound—a sponge was thrust into it without ceremony. About the sixth day he had violent hemorrhage; the gentleman present, not attributing the accident to the

right cause, tied the artery higher up, and sagely intimated, when a recurrence of the bleeding from the artery took place, two or three days afterward, that we, who had applied the ligature for the operator had not applied it tight enough. Examination after death, and before any dissection was made, evinced that the limb supposed to edematous, was in reality in a state of mortification. We watched this case throughout its progress, and we feel no hesitation in saying that this patient lost his life in consequence of the division of the femoral vein. How far such a case would admit of remedy is involved in some doubt, but in a similar one we would endeavor to approximate the edges of the vessel by means of a suture or two of fine leather, and, if possible, dress without compression."

Dr. Jameson emphasized the importance of occluding the artery with the minimum damage to the part constricted, in order that he would injure the structure as little as possible by the ligature. He enters into a labored article to refute the teaching of Dr. Jones, to whom is given the credit of the still common practice of applying the ligature sufficiently tight to cause a rupture of the intima.

"We are now prepared to speak of the application of the ligature in cases of amputation. It has been acknowledged, by all surgeons, that common ligatures of silk or flax act as extraneous and irritating bodies in wounds, and hence the practice of cutting off one end. This was attempted to be improved by the fine ligature by Mr. Lawrence, etc.; but though we admit this was an improvement upon the old practice, yet it has not always succeeded; and the fact of its often cutting its way out is a proof of its liability to produce attrition and irritation. We have used the animal ligature in a considerable number of amputations of the limbs, twice in both limbs of the same patients, and in amputation of the mamma. In the latter cases we have, with few exceptions, healed the wounds by the first intention in fourteen or fifteen days; and in the worst cases we have

never seen irritation from a ligature, and never more than slight suppurations which delay the healing a few days. In cases of other amputations we have never seen anything of our ligatures; have never had one to slip off, and the stumps heal sooner than when common ligatures are used."

In the following quotation we find he faintly foreshadows the suturing of wounds, and learned by observations that a wound is much less liable to become infected when treated in the so-called open method; in the latter years advocated strenuously by our late friend, Dr. James A. Wood, of New York.

"We have had occasion, in depraved habits, to tie some of the larger veins; we have seen a number of veins pour out blood freely in such habits and have arrested it, by using the crooked needle, by means of which we included in an animal ligature a considerable portion of the surrounding structure, and our experience warrants us in saying that no injury will arise from using an animal ligature in that way, where it may be found necessary. We have never had occasion to open a stump on account of hemorrhage; such a thing, we believe, never having occurred where an animal ligature was well applied, and we have never known one applied otherwise. The other circumstance, to which we alluded, is that of exposing cut surfaces freely to the atmospheric air and never being in too much hurry in closing surgical wounds. For this we are indebted to J. Bell. We not only wait until the blood coagulated well over the face of the stump, but until there is a degree of action restored to the arterial system; about half an hour may be considered the proper time."

Dr. Jameson's opinion upon the application of the ligature in cases of wounds of arteries is of present interest, especially in the light of very recent observations upon the aseptic closure of wounds of large arteries by means of sutures. His deduction is that the ligature should be used at once rather than trusting to means of compression.

"*Of the Application of the Ligature in Cases of Wounds.* In the first lines of S. Cooper (152), reference is made to the experiments of Jones and Blecard, which were intended to prove that wounds of small dimensions, in large arteries, will heal without obliteration of the vessels. Mr. Cooper has, however, very properly remarked that it is a point settled by dissections, that no artery, wounded, and healed under pressure will remain pervious. These experiments, therefore, afford nothing applicable to practice. To trust a wounded artery to nature or to attempt the application of sutures with a view of closing such wounds will never be justified in the human body, and to attempt to close such wounds by compression, if the injured vessel be of considerable size, is to incur the risk of disappointment and danger which may be avoided by the application of the ligature."

In an illustration I quote further from his experience:

"We had a patient who had his forearm torn by a large stone falling on it; the radial artery was denuded and appeared much contused in fully an inch and a half of its length. The sheath was split up about three-fourths of an inch and an animal ligature applied to the sound artery, and the lower portion managed in the same manner. All that portion of the artery visibly injured was removed, as also all the contused cellular tissue. The wound, which was nearly in the longitudinal direction of the arm, was now carefully closed by adhesive plaster. Nothing was ever seen of the ligatures and nearly the whole of the wound healed by the first intention—a little pus issuing, for a few days, from the lower angle of the wound, the outer or superficial part of which closed before the first dressing. We also saw a case where, by the bursting of a gun, a large wound was made in the forearm by some part of the fragments. The artery was torn off and bled freely. The patient, however, by grasping the parts firmly was enabled, by the favorable course of the wound nearly in the direc-

tion of the radius but somewhat spiral, to prevent immediate death. A physician of experience in the neighborhood applied, shortly afterward, a tourniquet. When we arrived we dissected the artery clean, until satisfied of having a sound part, an animal ligature being applied and the tourniquet loosened. The lower portion together with two anomalous branches about half an inch apart, and the first as far from the radial, bled furiously. These were tied with the animal ligature; the injured artery and cellular structure were removed by the scalpel. The parts carefully brought together by adhesive plaster healed in much the greater part by the first intention, although the wound was contused and lacerated so that we had no expectations of such a result. The ligatures were never seen nor was there any pus from the wound. . . . . As a general rule, it may be said that, where we tie an artery that is wounded, we should apply two ligatures; but we have no hesitation in saying that if the animal ligature be used there is no advantage whatever in dividing the vessel between the ligatures. If there be severe contusion, which is likely to lead to sloughing, we should treat the contused part as we would those around; if much bruised cut them out."

In a further criticism of forceful ligation of arteries Dr. Jameson quotes the practice of Scarpa, which very naturally he criticizes, although he considers the practice superior to that of Dr. Jones, before referred to, who insisted upon the division of the interior coat of the vessel.

"We are not able, precisely, to state the views of Dr. Physick[1] upon this subject, but to him is certainly due the merit of introducing the animal ligature into practice. We will endeavor to do the doctor justice; if we fail in it, it shall not be wilful. We have been creditably informed that Dr. Dorsey, acting under the auspices of Dr. Physick, was in the habit of cutting his ligatures from chamois leather, and rolling them on a slab to make them round. This, to us,

[1] Philip S. Physick, born 1768, died 1837. Student of John Hunter.

is a clear proof that Dr. Dorsey, at least, did not coincide with Scarpa in opinion, but with Jones. And we know experimentally, whatever may have been the theory of those gentlemen, that a leather ligature rendered hard by rolling, or close drawing through our nails, is liable, if applied tight, to cut the inner coats. This practice, therefore, was not suited to the views of Scarpa, Crampton, etc.

"If we are right in the opinion which we have just expressed, Dr. Physick is entitled to the credit of bringing into use the best ligature, as to the material, but here his claim is at an end. The following results seem to have been attained: The experiments of Hunter and Jones show the importance of the vasa vasorum, and consequently the advantage of leaving arteries to be tied, as much as possible, *in situ*. Scarpa and his associates, in opinion, have shown the superiority of the method of applying ligatures which will not cut nor bruise the coats of the artery; and the advantage of removing them before the suppurative inflammation is produced by their presence. Dr. Physick and Sir Astley Cooper have shown the advantages of using a substance which will serve as a ligature until the artery is obliterated, and be speedily afterward in the power of the absorbents so that they will remove it. Now, in the practice of Hunter and Jones, although they were aware of the importance of preserving the vasa vasorum, yet, we see them cutting off the true vasa vasorum and relying chiefly on the vessels subsidiary to them. The disadvantages of this method, then, are that they weaken the arterial coats, destroy the circulation in the true vasa vasorum, and thus produce too much inflammation. This method will nevertheless succeed very well in vessels of moderate size, but in the larger arteries it will not, because large vessels when tied fall into puckers, the coats are cut irregularly, the cellular coat is too weak to confine the tissue arterial, and consequently its elastic power will raise some of the cut folds above the ligature, drawing the yielding external coat

with it; the sides of the vessel between the ligature and the heart will not be in contact, no union will take place, and secondary hemorrhage will, sooner or later, follow the use of such a ligature. We have two cases in point: those of Dr. Mott and Dr. Graeffe, upon the arteria innominata. In the practice of Scarpa and others, pointed out above, all the objections to that of Jones are avoided, hence the preference of the former over the latter. But strong objections grow out of the circumstance that we cannot heal the wound by the first intention. On the fourth day we are to cut the ligature and remove it. This part of Scarpa's practice is exceedingly objectionable because the wound must be kept open, and to effect this we must often introduce some extraneous body into the wound to prevent its healing before the fourth day; and no dexterity of the surgeon, nor ingenuity in the construction of an instrument to effect this purpose, can be devised which will obviate considerable pain to the patient, and perplexity and apprehension to the surgeon in cutting the ligature. We have cut for the femoral artery in a patient in whom there was fully one inch of fat over the artery; in such, and similar cases, there would be much difficulty in cutting a ligature.

"A decided preference must be allowed to the method of Physick and Cooper, as it involves facts and principles which free it from all the objections to which the methods of Jones and Scarpa are liable. But, notwithstanding the facts and prinicples afforded by these celebrated men, they have left the subject susceptible of improvement; and to point out, and establish upon substantial and solid ground, some material improvement in this department of surgery has been the chief aim and design of this essay.

"We will now proceed to point out our own views and endeavor to support them by experiment.

"1. We believe that if an artery is sufficiently healthy to admit of its obliteration by adhesion of its sides, it is best done by a ligature which will neither cut its coats nor strangu-

late, except in parts, the true vasa vasorum, so that the continuity of the vessel shall not be destroyed although we obliterate its calibre.

" 2. We believe that if an animal ligature of the proper kind be properly applied, the vessel will be obliterated, the wound may be healed by the first intention, and the ligature will not cause suppurative inflammation; but in due time, being dissoluble, the whole will be removed by the absorbents. There will be no breach of continuity in the artery, the materia arteriarum will be removed, and the vessel, which during the state of inflammation and effusion of lymph was converted into a cord, will very soon afterward be resolved into a flat string of white cellular structure.

" EXPERIMENT I.—The carotid artery of a sheep was laid bare, and a common suture needle was passed through the artery armed with a tapering ligature the largest end of which was about three lines in width and its thickness that of very thick buckskin. It was cautiously drawn through nearly to the largest end, both ends were then cut off about three lines from the vessel; not a drop of blood escaped. The animal was killed, twenty-two days after the experiment, in good health. The artery was greatly thickened in its coats about an inch above and below the seton. No traces of a wound could be seen on one side of the artery; on the other side there was a slight depression in which lay a little yellowish matter, but which on examination proved to be a small part of the buckskin remaining within the vessel and reduced to a pulp by its mixture with a portion of lymph. The sides of the vessel were closely in contact but not quite united; a flattish opening was seen, through which a very small probe might have been passed. There is no doubt that this vessel, which was thus gradually contracted in its calibre, would in a short time have been quite obliterated, and we are quite certain no blood circulated through it at the time the animal was killed.

"EXPERIMENT II.—The jugular vein of a sheep was surrounded by an animal ligature, rendered firm by drawing it, after cut from the skin: at the end of six days the animal was killed. The ligature was still around the vessel, but upon scraping it gently with a knife the skin was found as thin as tissue paper. The vein lay in puckers or plaits within and there was no union of the coats under the ligature. Above the ligature the vessel was inflamed and distended by a coagulum which adhered to its sides, and was about two inches long. The vein became thickened a little below the ligature and assumed the size of the vein when distended with its blood, but was solid. When cut across it had the appearance of a transverse section of the human esophagus; though traces of its calibre could be seen, yet it would not admit the end of a small blow-pipe. No pus in any part, but lymph still exuding. In short, the vessel was completely obliterated without union of its sides, but by thickening of the coats, as represented by Mr. Travers. It is evident here there would have been no breach of continuity of the vein, and in a few days the small remains of the ligature would have been removed.

"EXPERIMENT III.—The left carotid of a dog was laid bare and transfixed with a buckskin string, as in experiment I. The animal continued playful and although there was considerable swelling of the throat it ate well every day. This animal was killed on the twenty-fifth day after the operation; no traces of violence remained in the parts until we came down upon the artery, which was slightly contracted, somewhat thickened, and hardened in its coats. Small red dots were seen here and there within the artery, and on the side next the heart the inner coat was a little fuzzy. On either side of the vessel were seen two little yellow knobs, which proved to be the two ends of the ligature, in the state of yellow pulp. The knobs were completely enveloped in a strong membraneous capsule; the same capsule enclosed both knobs, and its fibers were plainly seen crossing over the

artery. This arrangement of the capsule seemed to have the effect of drawing the button-like knobs together and was thus closing the vessel. If any circulation had remained it must have been very little, as the artery was so contracted just where the string passed through it as to be in contact with the inside coat of the artery. On the lower side, where the coats were not cut in splitting open the vessel to examine its interior, there was an actual adhesion of the leather to the inner coat. The string was softened into a pulpy consistency, and near the inside of the vessel was evidently covered with organized lymph, and extending along its edge at one side some distance. When the string was gently pulled one plainly saw a delicate membrane passing off from the artery and ending on the string in fringed extremities. This attachment of the vessel, by a new formation, existed on both sides of the artery and also at its inner and outer sides. The capsule covering the knobs or ends of the string was fully equal in strength to the outer coat of the artery and, therefore, there was no tendency to hemorrhage.

"EXPERIMENT IV.—We tied the left carotid low down in a sheep; at this part the vessel lay deep and a good deal of violence was done to the parts before the artery was fully exposed. It was detached from its connections about an inch and a half. This animal was extremely wild, seemed unwell, and would not eat freely before operated on, and scarcely at all after. An animal ligature was applied tight. On the sixth day this animal died; the parts were in a state of gangrene and, therefore, little information is to be had from our examination.

We remarked that the ligature was very tight, and had cut the inner coats nearly all round; no adhesion had taken place. We cite this case to show that a *drawn* buckskin ligature will cut the inner coats, and to show that where the inflammation runs too high for the production of lymph the ligature will not readily dissolve.

"EXPERIMENT V.—We exposed the jugular vein of a sheep, which was unusually large, say three-fourths of an inch in diameter. A tapering string of soft buckskin was passed through it as in experiment I. At the end of six days this animal, being the subject of experiment IV, died. On examination the vein was affected with gangrenous inflammation, but had contracted at least two-thirds of its size where transfixed. Some lymph, of a reddish color, surrounded the ligature, giving it much the appearance of a pulpy mass of flesh. The sides of the vein closely embraced the leather, and there were plainly to be seen, inside of the vein, membraneous shreds passing off along the string. This vessel would doubtless have been obliterated, and no risk of hemorrhage existed.

"EXPERIMENT VI.—We tied the jugular vein of a dog with a drawn animal ligature. At the end of nine days the animal was killed and the following appearances presented, the animal not having been sensibly injuried in its health: The vein immediately below the ligature, though somewhat puckered, was healed by adhesion of the inner coat according to the views of Dr. Hunter; above and below, for nearly an inch, the coats were greatly thickened. The inner coats were divided and the two ends of the vein separated more than half an inch. There was no coagulum on either side of the ligature, nor any appearance of inflammation. About half an inch of the vessel, on both sides of the ligature, was greatly thickened. The ligature below the vein was completely removed, and a pulpy, yellowish, little, button-like fragment of the ligature remained on either side of the vessel, but detached from each other and surrounded by a quantity of lymph; no appearance of pus. The parts around were not inflamed nor diseased. The appearances in this case exactly resembled the drawing in Mr. Travers' thirteenth plate.

"EXPERIMENT VII.—The superficial femoral artery of a sheep, a vessel about the size of a crow quill, was dissected up a small distance and torn out. The animal did not seem to

mind it; there was but two or three teaspoonfuls of blood. We found this artery contracted much more than a dead artery, and it had a sort of writhing motion when handled, convincing us of its vitality. When examined after the death of the animal, the artery was filled by a coagulum about an inch long, and it was contracted and hard like the vas deferens; the cellular tissue around it somewhat injected with blood, but not so much as to produce any pressure upon the artery.

"EXPERIMENT VIII.—A number of experiments were made upon a dog which had been bled to death the day before; ligatures of various kinds were applied from just below the arch of the aorta to near the iliacs, and the following results were obtained:

"1. Ligatures of four common coarse sewing threads, twisted slightly into a cord, would not cut the inner coats.

"2. Ligatures of two threads applied tightly not only cut the coats entirely around, leaving nothing but the thin outer coat, but by cutting the vasa vasorum a small portion of blood escaped and formed a red line which appeared on removing the ligature.

"3. Buckskin ligatures of common size, *drawn through the nails of the thumb and forefinger,* cut the coats more or less.

"4. A flattish ligature *about two lines in width, not drawn through the nails but applied tight,* neither cut nor bruised the inner coats.

"5. Ligatures of buckskin of common size, *not drawn, applied gently,* did not cut the inner coats. But all kinds of the leather ligatures could be applied, with care, tight enough to bring the sides of the artery into contact without cutting the coats. In every instance the vessel was much puckered, but still the sides in all cases lay pretty well in contact, but when tied with the cutting ligature were no better than when they did not cut. Indeed we thought there was a preference, in this respect, in favor of the soft animal ligature loosely applied. We have practised these

experiments upon the large arteries of human subjects and find no difference, except that the coats of the human kind do not so readily give under the animal ligature, and with any tolerable prudence in the application they will not cut the inner coats at all.

"We trust we may now venture to assert that we have arrived at the following important conclusions: (1) Hunter and Jones were right in advising us to take care of the vasa vasorum; (2) Scarpa, Crampton, Travers, etc., were right in objecting to cutting the *tissue arterial;* they did not succeed, however, in devising the proper means of securing the intention of obliterating vessels without destroying their continuity; (3) Physick and Cooper, by employing a ligature having the property of dissolubility, greatly improved our practice, whatever might have been their theory. Physick's ligatures, first used in 1814, have done much good. Cooper by tying, in 1817, the femoral artery of a man aged eighty years, and healing the wound by the fourth day, shows the superiority of this method over that of Scarpa.

"We aim at a combination of the improvements of all these parties; we say, do not disturb the vessels subsidiary to the vasa vasorum; do not cut off the true vasa vasorum nor strangulate them by your ligature. Tie the artery with a very soft buckskin ligature, and a little broader than the thickness of the skin; take care not to tie it too tight. Having done this you may expect that the wound, in many cases, will heal by the first intention; where this fails there will be no suppuration about the vessel and the wound will close very soon. You may expect a capsula to surround your ligature if you have not disturbed the capillary vessels more than can be avoided, or the vessel will be surrounded by an abundance of lymph and the ligature dissolved.

"In support of the above declaration we have to offer, in addition to the foregoing experiments, the following results of our surgical practice: We have used the animal ligature for six or seven years; we have used it in many amputations of the limbs and of the mamma; we have once tied the

carotid; once the external iliac; twice the femoral; four times the radial; once the posterior tibial; seven times the spermatic; and in no instance have we ever had secondary hemorrhage; never have we had a ligature to slip; we have never seen anything of our ligatures after their application, and often, indeed generally, our wounds have healed by the first intention. . . . . In cases of amputation we believe that no ligature will bear a comparison with one of soft buckskin, of moderate thickness, cut from the skin about twice as wide as thick, and drawn between the nails of the thumb and forefinger so as to make it more solid, but it should never be made round; this would cause it to cut the coats, make it less dissoluble, and more apt to slip off, all of which we ascertained by actual experiment."

In conclusion, it is interesting to note that Dr. Jameson was decidedly opposed to the then common habit of a reduced regimen by the use of calomel and other cathartics, a rigid diet, etc., and he totally forbids the use of all spirituous drinks of every kind, at that time considered a *sine qua non*. He concludes by saying: "We must content ourselves, here insisting upon the facts, whatever may be thought of our theories."

Thus it will be seen that to Physick, of Philadelphia, who about 1814 first used the animal ligature, and to Jameson, of Baltimore, are due this innovation in surgery, the practice of which is now in daily use on both continents. We trust the time is not far distant when proper honor may be accredited to these two great men and through them to American surgery. They laid the foundations for the introduction and use of the absorbable animal suture which now justly holds a first place in modern aseptic surgery.

In this connection I am sure it will be *apropos* to refer briefly to my own experience in the use of absorbable ligatures and sutures.

It was my good fortune to be the special pupil of Mr. Lister in 1869. He had only recently introduced the animal ligature for the occlusion of vessels in surgery. This was based

upon original studies which, owing to the laws of England, he was obliged to resort to the Continent to carry into effect. His inspiration grew out of his studies of Pasteur's demonstrations of the laws governing fermentation; that it was a vital and not, as Liebig had declared, a chemical process. If vital, why not the explanation of the hitherto unknown vexed problem of wound inflammation? Out of this grew, as you all know, the choice of catgut rendered antiseptic by steping in carbolic acid. His experiments duplicated and his results paralleled, in very large measure, those of Dr. Jameson, although I have every reason to believe that Mr. Lister was entirely ignorant of them. Mr. Lister had never used buried animal sutures, although he had often called my attention to the fact that a through-and-through stitch tied in the usual manner, in the processes of primary union, was frequently separated in the deeper layer of the skin, leaving the lower portion of the loop permanently buried. To me it seemed a logical deduction that the tissues could be coapted safely by buried sutures, the changes resulting in and about the suture going on precisely as when placed to include only a vessel. The results of my studies were first published in 1870, followed by many subsequent publications which are now well known. For very many reasons the sutures from the tendons of animals, especially those from the tail of the kangaroo, are preferable to catgut.

Long-continued careful, experimental, research work upon the lower animals enabled me to thoroughly demonstrate that the aseptic animal connective tissue, whatsoever its special character, whether applied for the constriction of vessels or used as sutures for the coaptation of well-vitalized structures, was first surrounded by leukocytes. These slowly invade the foreign material and, *pari passu*, as absorption takes place, they are transformed into connective-tissue cells, largely replacing the suture material. Thus there results a living band, more or less permanently replacing the suture material. Such reinforcement of the structures, after many operations

as for example hernia, has been proven to be of great practical importance. The change of a well-established custom or practice is necessarily slow, but one ventures little in the assertion that the absorbable animal ligature and suture will retain a permanent place in surgery.

## DISCUSSION.

Dr. I. S. Stone, of Washington, D. C.—It affords me great pleasure to name an old friend of mine who succeeded, by some years, the gentleman who has just been mentioned (Dr. Jameson), as health officer of the city of Baltimore, Dr. William M. Kemp. I have heard that in 1845, when an epidemic of yellow fever took possession of Norfolk, he, as health officer of Baltimore, disbelieved in the contagion of yellow fever, and opened the doors of this city to refugees from Norfolk. In not a single instance was the disease acquired by a resident of Baltimore.

Dr. R. M. Cunningham, of Ensley, Ala.—I desire to address a few remarks to the subject-matter of such papers as this. I believe they are good, and of all professions in the world which should have a historian to place upon permanent record the work done by members of the profession by their succeeding brother practitioners, it is the medical profession. I could not help but be impressed today with an example of exact science of our profession. We have gone beyond the speculative, the dogmatic period of our profession; yet it is refreshing for a historian, such as Dr. Marcy, to go back into the past and find the empirical teachings of a great many of the then masters of the profession who have been confirmed in the chemical, biological, and pathological laboratories. The reasoning of the men in those days was wholly by conclusion, sometimes without fact, without any detailed statements, such as we now have. The public is ignorant of the great men of the medical profession.

Speaking of the ligature reminds me of Ambroise Paré. I venture the assertion that, among the educated people of Baltimore, few have heard of Ambroise Paré unless they have read Alexander Dumas' works, and have found that he was the physician to Charles IX. How many have heard of Sims and of Gross, of Jenner and of Hunter, etc.? We are living in an intensely practical age, and there are two dangers which beset our age. One is forgetfulness of the past and the other is regardlessness

of the future and present generations; that is, there seems to be little reverence for ancestors. There is a disregard for the less important things and events of history, and there is a tendency to allow posterity to work out its own salvation. I believe, therefore, that such addresses as this are good, and I wish to thank Dr. Marcy personally for introducing the subject-matter for discussion.

Dr. Marcy (closing).—The subject I have spoken to you about is a large one, and I cut out much in my paper that I consider of a great deal of value. If you will look over Dr. Jameson's experimental studies you will find that they were made with the same care as experiments of today, and one can see how thoughtful he was in drawing his conclusions from results obtained. He showed how the buried ligature was closed in by an infusion of lymph which, being transposed into connective tissue, made a vitalized band around the artery. Dr. Jameson's teaching was not lost upon his generation, and only lost in the pages of history subsequently by men who did not know of, or realize the importance of his work. Sir Astley Cooper showed that arteries were occluded by the buckskin ligature, cut short, and followed by primary union. Dr. Jameson was educated in London. McDowell received his education in Edinburgh. Dr. Young taught Dr. McDowell in Edinburgh that it would be an excellent thing if he used the animal ligature, and his first ligature applied to the pedicle of an ovarian tumor was a piece of buckskin. Hence, this history was not altogether lost in that generation and we should profit by those who have preceded us. In this connection it is hardly necessary for me to call your attention to what I consider a better ligature, that which you all know is now in general use, the kangaroo tendon. I take pleasure in asking of you the examination of the kangaroo tendon sutures which I here present. You will note they are long, fine, even, strong, easily sterilized, and are in every way superior to catgut. They hold at the knot perfectly and when not chromicized absorb rather slowly. They are best chromicized to remain subject to little change in the tissues for about two weeks. Much of the tendon that is on the market is bad and that is the only reason the tendons are now prepared for general use under my own supervision. I believe that the best contribution which I have made to surgery is the buried animal suture, and that it will not go into oblivion, as the ligature of my distinguished predecessor, Dr. Jameson, of Baltimore.

# THE SURGICAL ASPECTS OF GASTRIC CARCINOMA.

By John B. Deaver, M.D.,
*Philadelphia.*

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Cancer of the stomach is a disease of frequent occurrence. From 25 to 40 per cent. of all cancers in the body are primary in the stomach. Statistics[1] show that in Baltimore there are over 200 deaths annually from cancer; in Boston there are over 300, in Philadelphia nearly 500, and in New York nearly 900 deaths every year from some form of cancer. Of these deaths more than a fourth, and perhaps almost a half, are directly due to cancer of the stomach. Dowd[2] says that the United States Census Report for 1900 recorded 9000 deaths from cancer during that year, that 31 per cent. of fatal cancers were in the stomach, that probably many cancers of the liver and the abdomen were primarily gastric, and that many cases of gastritis and allied diseases were really carcinomatous in nature.

Medical measures are powerless to cure cancer in any region of the body. Escharotic pastes, cauterizing applications, detergent washes, serums and antitoxins, and even the subtile power of the *x*-rays and the Finsen light, are all of them powerless to cure external cancer. How much more improbable it is that any save surgical remedies will cure cancer ofthe internal organs! That, in the case of inoperable tumors, medical measures will sometimes prolong life and mitigate suffering, no one would be so foolish as to deny.

[1] Bryant, quoted by Van Valzah and Nisbet, Diseases of the Stomach, London, 1900, 513.

[2] New York Med. Record, 1906, i, 91.

The wide experience of Jacobi,[1] so cautiously and conservatively expressed, in regard to the beneficial effects of methylene blue (methylthionin hydrochloride) in such patients, is a gratifying example of what may be done by medical science even for hopeless cases. But, as Bland Sutton[2] said a number of years ago, as long as we are ignorant of the cause of cancer, so long must the only successful treatment be the extirpation of the growth; and I may add that the sooner this is done the better for the welfare of the patient and for the reputation of the family physician and the surgeon.

If it were possible to prevent cancer, what then? Before answering this question one would have to determine the cause of cancer; and of its specific cause we are absolutely ignorant. That it is caused by some form of microörganismal life it seems safe to infer; but beyond such an inference even the most enthusiastic pathologists will not go.[3] But we do know many of the predisposing causes of cancer, and especially well recognized are the predisposing causes of gastric cancer, which is all that concerns us now.

Mumford and Stone[4] traced 60 patients who had been treated at the Massachusetts General Hospital for chronic indigestion, and who subsequently died. Of these, no less than half died of gastric carcinoma. They further made a study of 50 patients with gastric carcinoma, and learned that in no less than 41 of the 50 patients there was "a history either of ulcer or of long-continued digestive disturbance, of which the exact nature could not be ascertained." Mayo[5] found a history of ulcer or other disease in 36 per cent. of his patients with cancer of the stomach, and clear evidence of cancer developed on ulcer in 30 per cent. of the last 40

[1] Jour. Amer. Med. Assoc., 1906, xlvii, 1545.

[2] Tumors, London, 1901, preface.

[3] See Gaylord, American Practice of Surgery, edited by Bryant and Buck, New York, 1906, i, 403.

[4] Surgical Aspects of Digestive Disorders, New York, 1905, pp. 87 and 201.

[5] Jour. Amer. Med. Assoc., 1905, ii, 1211.

pylorectomies and partial gastrectomies. Moynihan[1] noted a history of gastric ulcer or chronic indigestion in 27 out of 45 patients with cancer of the stomach (60 per cent.). Among 22 patients with gastric carcinoma, so far advanced as to be operable only by gastroenterostomy, no less than 16, or over 72 per cent., gave such a history. Jedlicka[2] quotes Ssapesko as stating that among 100 cases of gastric carcinoma only 10 were found in which the cancer was not engrafted on a preceding ulcer. Further statistics could no doubt be quoted; they would merely serve to emphasize the point that I am trying to make, *that chronic gastric dyspepsia is the chief predisposing cause of cancer of the stomach.*

We know that cancer of the stomach presents clinically two forms. In one, a patient past middle life, without having suffered previously from indigestion, suddenly loses appetite especially for meats, grows progressively weaker and more emaciated, develops epigastric pain and possibly a palpable mass, becomes subject to vomiting spells every few days, which bring up a mass of coffee-ground material, foul smelling and fermented, and quickly develops the cancerous cachexia. This is the classical picture of gastric carcinoma, and it is still seen; but it is of rarity when compared with that other course of development which we have only begun to appreciate in the last few years. This second and much larger course is found in patients who have been life-long sufferers from dyspepsia. Medical treatment has at times given relief, but the indigestion recurs again and again. Appetite may be preserved, but digestion is torture; so abstemiousness becomes second nature. There may or there may not have been some period when gastric ulcer was suspected or actually diagnosticated. Usually there have been no very definite symptoms, and the patient has been treated for chronic gastritis. Finally these patients die, worn out with suffering, with prolonged starvation, and wearied with

[1] Trans. Clin. Soc., London, 1906, xxxix, 84.

[2] Operat. Behandl. d. Chron. Magengeschwürs, Prag., 1904, 24.

ineffectual drugging. At the autopsy a wide-spreading epitheliomatous ulcer is found, distorting the stomach, stenosing the pylorus, and binding the colon, the pancreas, the stomach, and perhaps the liver and the gall-bladder together in an indistinguishable mass of adhesions. The wonder is that such patients live so long.

Experience in the surgery of the upper abdomen within the last few years has taught surgeons much, but they have learned nothing of more importance than the security and certainty with which mechanical defects in the physiological action of the stomach may be remedied by mechanical means. If chronic and rebellious cases of indigestion were promptly turned over to the surgeon, and if suitable operations were done on such patients, there would be fewer cases of carcinoma of the stomach observed by physicians. Every case diagnosticated certainly as carcinoma of the stomach before operation is a disgrace to the attending physician, provided he has had the patient under treatment for more than a few weeks. Patients who present symptoms of gastric indigestion which will not yield in that time to medical treatment should be subjected to operation; and if operation is not undertaken, and the patient's disease is allowed to continue uncured until a diagnosis of cancer has been made, the disgrace and the responsibility rest on the attending physician. We know beyond peradventure that such patients as these can be cured by operation, and that the development of carcinoma may be thus prevented. For physicians not to recognize this fact and act accordingly is not only unjustifiable, it is criminal.

The point is now reached where it will, I think, prove beneficial to compare the results of operations on the stomach for benign disease with those for carcinoma of this organ. It is of interest first to know the immediate operative mortality, and then the proportion of ultimate recoveries or cures. In considering questions such as these it is injudicious to collect isolated operations reported at random by different surgeons. Statistics of abdominal operations which are so frequently

done as are nowadays operations on the stomach present, when thus collected, a picture which is too likely to be distorted one way or the other: we obtain results which are neither phenomenally good nor phenomenally bad, but at any rate results which are far from truth. In the earlier history of an operation such figures are of value, because the cases are few, and progress can only come when a sufficient number of observations has been collected for surgeons to study the principles which underlie the operation in question. Thus, twenty years ago it was important to record every individual case of gastroenterostomy or pylorectomy, because no individual surgeon had at that time had the opportunity personally of observing the effects of more than one or two such operations. But now, when the experience of one surgeon may embrace a hundred or more such cases, the deductions to be drawn from such a series far outweigh in value those which were available even one decade ago from a series of similar size collected from various sources.

Let us consider, then, first the figures of gastroenterostomy for benign disease, an operation which may, I think, justly be considered as preventive of cancer. We have reports of large series of cases of this operation at the hands of various surgeons:

GASTROENTEROSTOMY FOR BENIGN DISEASE.

| Operator. | Cases. | Mortality. Per cent. |
|---|---|---|
| Robson and Moynihan[1] | 218 | 3.2 |
| Mayo[2] | 81 | 1.23 |
| Czerny[3] | 83 | 4.8 |
| Mayo Robson[4] | 97 | 1.03 |
| Moynihan[5] | 171 | 1.00 |
| Deaver[6] | 40 | 2.5 |
| Morison[7] | 27 | 3.7 |
| Hartmann[8] | 47 | 6.3 |

[1] Surg. Treat. Diseases of Stomach, New York, 1904, p. 177.
[2] Jour. Amer. Med. Assoc., 1905, ii, 1211.
[3] Petersen u. Machol, Beitr. z. klin. Chir., 1902, xxxiii, 297.
[4] Brit. Med. Jour., 1905, ii, 762.
[5] Lancet, 1905, ii, 362.
[6] Therapeutic Gazette, January 15, 1906
[7] Brit. Med. Jour., 1905, ii, 777. [8] Ibid., 772

Now, it is a well-recognized fact that about 95 per cent. of patients who survive operation for benign disease remain well permanently. Consider these figures:

End Results after Gastroenterostomy for Benign Diseases of the Stomach.

| Operator. | Cases traced. | Permanently cured. | |
|---|---|---|---|
| Barling | 7 | 7 | 100.0 per cent. |
| Mayo | 28 | 27 | 96.4 " |
| Moynihan | 29 | 29 | 100.0 " |
| Czerny | 53 | 44 | 83.0 " |
| Robson and Moynihan | 20 | 20 | 100.0 " |
| Paterson (collective statistics) | 116 | 109 | 93.9 " |
| Deaver | 30 | 30 | 100.0 " |
| Robson | 96 | 89 | 92.7 " |

Note.—The latest statistics of Mayo in gastroenterostomy for benign disease give a mortality of practically 1 per cent.: actually 1.04 per cent.—2 deaths in 192 cases. My latest statistics show a mortality of less than 2 per cent.

May we not justly conclude that by timely resort to this operation many patients have been saved from the development of gastric carcinoma?

The cure of carcinoma of the stomach, when this dread disease has once developed, demands excision of the growth with the infected glands. The immediate operative mortality of partial gastrectomy is still very much higher than is that of the operation I have been considering, though the death rate has been reduced very much within the last few years.

PARTIAL GASTRECTOMY FOR CARCINOMA.

| Operator. | Cases. | Deaths. | Mortality. Per cent. |
|---|---|---|---|
| Mayo[1] | 81 (25) | 12 (1) | 14.15 (4) |
| Kocher[2] | 110 (58) | 27 (9) | 24.5 (15) |
| Mikulicz[3] | 100 | 37 | 37.0 |
| Mayd[4] | 25 | 4 | 16.0 |
| Krönlein[5] | 50 | 14 | 28.0 |
| Roux[6] | 39 | 13 | 33.0 |
| Meyer[7] | 5 | 1 | 20.0 |
| Kappeler[8] | 30 | 8 | 26.6 |
| Garré[9] | 26 | 7 | 26.9 |
| Hartmann[10] | 22 | 7 | 31.8 |
| Moynihan[11] | 9 | 2 | 22.2 |
| Paterson[12] | 4 | 1 | 25.0 |

NOTE.—Mayo's latest statistics, as given in a personal communication, show a mortality of 11.45 per cent. for partial gastrectomy; while my latest statistics show a mortality of 11.11 per cent.

Taking, then, the more favorable recent figures of Mayo (4 per cent.) and of Kocher (15 per cent.), which offset to a certain extent the rather ancient statistics of Mikulicz, Krönlein, Roux, Hartmann, and others, we have a total of 393 partial gastrectomies with 104 deaths, or a mortality of 26.5 per cent., as the immediate result of operations for the radical cure of gastric carcinoma. This, it will be remembered, is to be contrasted with an immediate mortality of about 5 per cent. after operations for the prevention of this disease.

It will next be proper to determine what proportion of the 74 per cent. of patients who survive gastrectomy may reasonably hope to be ultimately and permanently cured of their disease. Let it not be forgotten that about 95 per cent. of patients surviving the prophylactic operation have been proved to be permanently cured. This question of the ultimate results of gastrectomy has been carefully studied by Moynihan,[13]

[1] New York State Jour. of Med., 1906, vi, 63.
[2] Paterson's Gastric Surgery, New York, 1906, p. 104.
[3] Paterson, loc. cit., p. 104.
[4] Paterson, loc. cit.
[5] Paterson, loc. cit.
[6] Paterson, loc. cit.
[7] New York State Jour. of Med., 1906, vi, 68.
[8] Paterson, loc. cit.
[9] Paterson, loc. cit.
[10] Paterson, loc. cit.
[11] Trans. Clin. Soc. London, 1906, xxxix, 84.
[12] Gastric Surgery, New York, 1906, p. 178.
[13] Abdominal Operations, Philadelphia, 1905, p. 223.

who bases his conclusions largely on the statistics of Krönlein and Mikulicz, who alone of many operators have published in detail the terminal history of those patients whom they were able to trace. It is not the place here to discuss these statistics minutely, but it may be said that they show that patients suffering with gastric carcinoma have, under medical treatment, an expectation of life of about twelve months from the beginning of the disease; many writers put it at less. Not only is this the duration of life, but it should not be forgotten that at the end of that time the patients under medical treatment will *all be dead*. There will not be even one who at the conclusion of that period will have had up to that time no recurrence of the growth, and who may, therefore, still be considered as curable—as potentially cured. If then surgery can show any permanent cures, and if the average duration of life under surgical treatment is longer, or at least not less than that under medical treatment, the conclusion is surely justified that surgical intervention is best for these patients.

To compare with an expectation of life of about twelve months for patients who have not been operated on, we know from Moynihan's studies that the average duration of life in patients who underwent gastrectomy in the clinics of Krönlein and Mikulicz was, from beginning to end of the disease, as much as twenty-four to twenty-six months—more than twice as long as the average duration of life without operation. Paterson's figures[1] from collective statistics give the duration of life after operation (86 operations in all) as an average of nineteen months after total gastrectomy (17 patients), of twenty-two and a half months after subtotal gastrectomy (14 patients), and of just over two years after partial gastrectomy (55 patients). It should be noted that Moynihan's figures refer to duration of life from the first symptoms of gastric cancer, while Paterson's refer only to the duration of life after operation. With this allowance, it is seen that the figures agree very closely; and as they are gathered from very

[1] Gastric Surgery, New York, 1906, pp. 106 and 107.

different sources (Moynihan's from the German clinics and Paterson's largely from British sources), each series serves to confirm the other.

Not only, moreover, may the mere addition to the patient's life be of utmost importance from a social, commercial, or financial point of view, but death, when it does come, will attack the sufferer in a less hideous form. The patient will not die of starvation, as he would have done had no operation been performed. And although it would be heartless in the surgeon to tell his patient, when urging operation, that although the chance of permanent cure is slight, it is nevertheless altogether likely that he will die from cancer of the liver, with its attendant cachexia, and not from progressive starvation—although, I say, to draw such a picture of the future for his unfortunate patient would be diabolically cold-blooded in the surgeon, yet I doubt not that many a patient, dying of the cancerous cachexia some two years after the operation, will daily bless the art of surgery which has so prolonged his life and mitigated his suffering during the gradual approach of death.

But, though the chances of permanent cure are slight, they are not altogether imaginary. Among more than 100 cases of gastrectomy for carcinoma, Kocher[1] has had permanent cure result in 8 patients, who were reported as living and well at periods varying from three to sixteen years after the operation. Paterson[2] has collected, mainly from British sources, the end results of 79 cases of gastrectomy, 46 patients (58.4 per cent.) dying of recurrence within three years, and 33 patients (41.6 per cent.) being alive and well at the end of three years or more after the operation. No less than 12 patients (15 per cent.) were in perfect health more than five years after the operation, some being in good health at periods varying from six to fourteen years after the operation. We may expect, then, about 10 or 15 per cent. of patients treated

[1] Revue de gyn. et de chir. abd., 1906, x, 103.
[2] Gastric Surgery, New York, 1906, p 108

by gastrectomy to be permanently cured without liability of recurrence.

PATERSON'S STATISTICS OF END RESULTS OF GASTRECTOMY.

| | Recovered. | Traced. | Died since operation. | Still living and well. Three years after operation. | Five years after operation. |
|---|---|---|---|---|---|
| Total gastrectomy . . | 17 | 14 | 5 | 6 | 3 |
| Subtotal gastrectomy . | 14 | 12 | 6 | 3 | 3 |
| Partial gastrectomy . . | 55 | 53 | 35 | 12 | 6 |
| | 86 | 79 | 46 | 21 | 12 |

There is another question of importance in regard to the surgery of gastric carcinoma which cannot be answered offhand. This is as to the advisability of excising ulcers or of doing a pylorectomy or a partial gastrectomy in the presence of benign disease as a preventive of malignant changes. I have argued in favor of gastroenterostomy as a prophylactic operation; and it may have been cause for surprise that I have not hitherto even mentioned excision of the ulcer or of the diseased gastric area. This omission, however, has been intentional, since I desire first to contrast the immediate operative mortality of the two forms of surgical treatment. And in doing this I am, of course, not ignorant of the fact that gastrectomy in a cancerous patient is unavoidably a more fatal operation than it is in one suffering only from benign disease. But although I am quite willing to admit this fact, yet the operation of partial gastrectomy presents *per se*, and apart from the vitality of the individual patient, certain added risks, which in the present state of surgery are unavoidable, but which must, I venture to think, be obviated before the conscientious surgeon can recommend this operation in preference to gastroenterostomy in patients with benign gastric disease. The time may come, indeed I hope it is not far distant, when the operative mortality of gastrectomy will be reduced from 25 per cent., its present figure, to less than 10 per cent., but until that day has been reached I for one cannot feel justified in resorting to the more dangerous operation in cases certainly benign. I say this, too, with

full knowledge and personal experience of patients who recover as blithely from a gastrectomy as they do from an interval operation for appendicitis. In my own experience with gastrectomy, comparatively small though it be, I have been surprised at the freedom from symptoms and the easy convalescence of some patients on whom quite extensive operations have been done; but, although increasing experience with the operation has made me regard it as neither so difficult nor so serious an undertaking as I formerly thought it, yet I have not yet reached, and I humbly trust never shall reach, that point where familiarity will breed contempt.

The arguments advanced in favor of excision of the ulcer or of the ulcer-bearing area, sound as some of them undoubtedly are, do not, in my opinion, outweigh the arguments against this treatment, which I have already stated. Though I firmly believe that the chief predisposing cause of gastric carcinoma is long-continued dyspepsia due to ulcer or other form of benign gastric disease, yet I do not altogether credit the reports which would make us believe that about one out of every ten ulcer cases is already cancerous.

Originally advocated by Rydgier,[1] who has recently[2] had the satisfaction of reporting his first patient as alive and well twenty-five years after the operation, pylorectomy was, nevertheless, a method which quickly fell into disrepute, owing to its enormous mortality. But in the last few years excision of the ulcer or of the pyloric region has again become popular with some surgeons (Maydl, Jedlicka, Ali, Krogius, Rodman, and others), chiefly as a prophylactic against the development of carcinoma, though there are also other reasons (absence of subsequent perforation or hemorrhage) which they urge for its more general adoption. Yet it seems to me that a diagnosis of "carcinomatous degeneration," based on microscopic findings, as to whose interpretation pathologists themselves are not unanimous, is at least as liable to be in error as is a clinical diagnosis of malignant

[1] Berl. klin. Woch., 1882, xix, 39. [2] Ibid., 1906, xliii, 1207.

disease based on the naked-eye appearance of a pyloric tumor found at operation; and it seems fair to conclude that patients in the former class might be as thoroughly cured by a palliative operation as have been those patients, by no means unknown to science, who presented a gastric tumor which was considered a naked-eye evidence of malignant disease too far advanced to be treated by excision.[1]

It is, therefore, with caution that I accept the conclusions of Jedlicka,[2] who argues very positively in favor of excision of benign ulcers. Following his late master, Maydl, he records, during the period from 1891 to 1904, 34 gastric or pyloric resections for benign disease, with 2 deaths, a mortality of a little less than 6 per cent. It is not his operative results that I mistrust, for they are brilliant and speak for themselves; but it is the conclusions which he deduces from a study of his cases that I think should be accepted with caution. He states that his microscopic studies (confirmed in most cases by Hlava) showed that of his patients whose stomachs were resected for gastric ulcer, supposedly benign, no less than 26 per cent. had commencing malignant degeneration. He points out, moreover, that of the 41 patients whose stomachs were resected, between 1891 and 1901, for carcinoma, not one is alive now; whereas, of the four patients whose stomachs were resected at the same time for supposedly benign disease, which was, however, afterward found by the microscope to be malignant, but in an early stage—that of these 4 patients, the first is well eight years after the operation, the second is well four years after the operation, the third is still too recent to count, and only one died of recurrence, and then only after two circular resections of the stomach. In all fairness I must add that Mayo Robson[3] has recently

[1] See cases reported by Bidwell, Amer. Jour. Med. Sci., 1899, ii, 260; Demoulin et Tuffier, Bull. et mem. de la Soc. de chir. de Paris, 1899, xxv, 833; Robson and Moynihan, Diseases of the Stomach, New York, 1904, p. 386; Wallis, Brit. Med. Jour., 1906, ii, 73.

[2] Operativ. Behandl. d. Chron. Megengeschwürs, Prag., 1904.

[3] First Congress of Internat. Soc. of Surgery.

reported, among 97 patients operated on by gastroenterostomy for benign disease, no less than 4 who developed cancer at periods of from one to three and one-half years after the palliative operation; and that Czerny's clinic[1] furnished 3 such results among 83 operations for benign disease. But then it must not be forgotten that if these patients had been treated by the more dangerous method of gastrectomy, it might have been their fate to succumb to the operation—a result immeasurably worse than the remote and improbable chance of carcinomatous change several years subsequent to operation. So that while it is right to acknowledge the force of the arguments advanced by the advocates of gastrectomy as a prophylactic measure, I think it is a saner course to limit the more dangerous operation of resection to the more serious condition of acknowledged malignancy, and when there is doubt as to the existence of malignant disease to perform the more extensive operation, not as a routine measure, but only in carefully selected cases.

There remains for discussion the choice of operation on patients who are undeniably affected with carcinoma. Until operation is more frequently undertaken in the very early stages of the malady, gastroenterostomy must still be the operation most often adopted. It has been rare in my experience, as I think also in that of other surgeons, for a patient in whom cancer has been certainly diagnosticated before operation to present conditions admitting of gastrectomy. The operations of gastrectomy that I have done myself have been on patients in whom the existence of malignant disease was merely suspected, but not certainly known, before the abdomen was opened. A palpable tumor felt before operation will naturally suggest cancer; but the case reports already referred to, in which such masses have been known to disappear after gastroenterostomy, prove that all palpable tumors are not carcinomatous in nature. I have urged else-

[1] Peterson u. Machol, Beitr. z. klin. Chir., 1902, xxxiii, 297.

where[1] that in this class of patients exploratory laparotomy finds its most legitimate field. The statistics from the clinics of Krönlein and Mikulicz, recently compared by Moynihan, show that patients who have undergone an exploratory laparotomy in which no further operative treatment was possible actually lived longer than did those whose disease was so far advanced as to make even an exploration unjustifiable, or those who entirely refused an operation of any kind. In very many operations, moreover, which are commenced as explorations merely, it is found possible either to remove the growth or, at least, to perform a palliative operation which will materially prolong life and relieve suffering. Surgeons, I fear, sometimes lose sight of the fact that this and not the production of statistics is the end and object of surgery.

We have a choice of radical operations for cancer of the stomach, dependent upon the extent of the disease. Total gastrectomy, though it has been adopted in no less than 27 instances, must remain, I think, an operation of the utmost rarity. Personally I do not approve it. Though 17 patients survived the operation (a mortality of over 37 per cent.), I think it probable that the mortality will rather increase than decrease if the operation be indiscriminately employed. Subtotal gastrectomy, as it is called by the French, is in my opinion as extensive an operation as it is wise to adopt. I have successfully (?) removed as much as two-thirds of the stomach, always leaving the cardiac end and the "greater tuberosity" untouched. The operation of pylorectomy, or partial gastrectomy as it is now more accurately called, is that form of excision which is most generally applicable. The mortality of subtotal gastrectomy was 30 per cent. in 20 cases collected by Paterson—all he could find records of; while the average mortality of partial gastrectomy for carcinoma is, as we have already seen, from 26 to 28 per cent.

Adhesions I think a more formidable objection to radical removal than size of the tumor or lymphatic involvement.

[1] New York State Jour. of Med., 1906, vi, 64.

Especially is this true when the adhesions involve the pancreas. Enlarged glands should always be removed when possible, but as they are frequently the seat merely of inflammatory, not of malignant, changes, their number need not discourage the surgeon from attempting radical cure by gastrectomy, provided the tumor is fairly movable. Thus Taylor[1] did a partial gastrectomy in a woman, aged sixty years, excising with the tumor *en masse* some of the right rectus abdominis muscle to which it was adherent, along with some enlarged glands in the gastrohepatic omentum. The patient recovered, and microscopic study of the tumor showed that it was of purely inflammatory character. Still more extensive operations have been performed when the growth was really carcinomatous. Moynihan[2] has twice (once with success) excised a tumor of the stomach with the corresponding portion of the transverse colon which was implicated in the malignant growth; and a similar operation has been successfully performed by Childe[3] (for unavoidable destruction of the blood supply, not for carcinomatous invasion), who also excised a layer of pancreatic tissue.

For the restoration of the gastrointestinal canal after gastrectomy I much prefer the usual posterior retrocolic gastroenterostomy to either of Billroth's methods, or to Kocher's operation of lateral implantation of the duodenum into the posterior gastric wall. In some patients, I think, it is wise to do this part of the operation first. Paterson also has recommended this course, and suggests that, while the gastroenterostomy is being done, a piece of the suspected growth can be frozen and examined under the microscope. The plan suggested a decade or more ago by Tupolske, of doing a gastroenterostomy at one sitting and postponing the gastrectomy for several weeks, is open to the objection, as Mayo Robson[4] has pointed out, that the patient may be so much

[1] Indian Lancet, 1906, i, 668.
[2] Trans. Clin. Soc., London, 1906, xxxix, 84.
[3] Brit. Med. Jour. 1906, i, 194.
[4] Ibid., 1900, i, 696.

relieved by the palliative operation that he will decline further treatment. In one of Paterson's patients, however, treated thus by a preliminary gastroenterostomy, good health persisted after four years; so it is probable that in this individual case it was wise for the patient to decline excision of the supposedly malignant growth.

When the carcinoma has progressed very far, so that not even gastrojejunostomy can be done, the operation of jejunostomy will relieve suffering and may prolong life. I do not think the profession in this country appreciate the degree of alleviation of suffering which this procedure affords. The fistula should be established by a technique similar to that employed by Witzel in performing gastrostomy. In such patients, after opening the abdomen, no time should be lost in idly exploring the stomach and duodenum. We should know before commencing the operation what it is desired to accomplish, and the primary loop of the jejunum should be immediately sought and attached in the manner advised to the anterior abdominal wall. I prefer the incision splitting the fibers of the left rectus muscle. Feeding should be begun almost immediately, in moderate amounts, such as would be given to a starving man under other circumstances. Rectal alimentation may also be employed, but, except for the saline solution which can be thus absorbed, it is of little value.

In conclusion, I may say:

1. By timely operation for the various causes of persistent indigestion, many patients will be saved from developing gastric carcinoma.

2. Early diagnosis of gastric carcinoma being so extremely difficult, and radical removal being only highly promising when an early diagnosis has been made, partial gastrectomy should be an operation limited to cases of malignant disease operated on for symptoms of pyloric obstruction or other gastric affection supposedly benign, in which the existence of cancer, though perhaps suspected, cannot be certainly diagnosticated before operation.

3. In cases of gastric carcinoma moderately advanced gastroenterostomy should be preferred to partial gastrectomy.

4. When the indication is to prevent starvation, jejunostomy should be performed.

## DISCUSSION.

Dr. W. D. Haggard, of Nashville, Tenn.—No paper has come before the Association that will be of more value than the one just presented by Dr. Deaver. I will say, in the nineteen years' existence of the Association, it is the first paper to my knowledge that has been presented on cancer of the stomach; and still cancer of the stomach kills more people than any other organ of the body, except the uterus, and cancer in general more than any other disease except tuberculosis.

The modern surgery of the stomach is only five years old, and Dr. Deaver is one of the great American surgeons who has done much to develop this important field of surgery, so that any statistics before that period must now be relegated to obscurity. We must look upon the operation for cancer of the stomach as being in the same category almost as the earlier operations for removal of the uterus. For instance, the modern operations for cancer of the stomach have been so perfected now that the mortality is only 4 per cent. in the hands of the most experienced and skilful surgeons, and if such a mortality can be obtained by the experienced surgeon, can we not expect what has been outlined in the paper, a mortality of from 5 to 10 per cent. in the hands of less experienced surgeons?

Of course, the great difficulty is in making the diagnosis. I regret to say that some specialists in diseases of the stomach have impeded any material advance in the methods of diagnosis. It should be borne in mind that the only certain method of diagnosis of cancer of the stomach at the present time is an exploratory incision. That should be accentuated more than has heretofore been done. Any elderly person suffering for a considerable time with persistent dyspeptic symptoms, unrelieved by medical treatment, should have it explained sufficiently so that he or she, as the case may be, will be willing to have the only means of diagnosis applied, namely, an exploration. It is the patient's prerogative to object to an exploration, and it is sometimes very hard to convince them of the necessity of it. There-

fore, it behooves us to educate the general practitioner to this important step, if we expect to make advancement.

A paper going out like this will do a great deal toward crystallizing the thought of those men who are treating cancer of the stomach by purely medical means. These men do not treat cancer in that way when it occurs in any other organ; but they treat their cases of cancer of the stomach until it is sometimes too late to do anything for them in a radical way.

I feel that we should add our voice and give our thanks to Dr. Deaver for bringing this subject before the profession at the present time. He has pointed out that early diagnosis is very essential, and has told us what brilliant results can be accomplished by radical excision of the stomach. (Applause.)

Dr. Charles W. Oviatt, of Oshkosh, Wis.—I wish to emphasize what Dr. Haggard has said, that surgeons should embrace every opportunity to impress upon the general practitioner not only the advantages of early operation in cases of cancer of the stomach, but his own individual responsibility in these cases.

Dr. Deaver gave us statistics in regard to the number of people who die each year in Philadelphia, Boston, New York, etc., from cancer. How many of these ever saw a surgeon or were ever taken to a surgeon? The trouble is these patients are treated as medical cases, as suggested by Dr. Haggard, and I think the general practitioner should be taught, as Dr. W. Mayo expressed it, to understand that if patients are allowed to go untreated by the surgeon, as, for instance, women who have nodules in the breast, from week to week, he should feel his responsibility if death occurs. In other words, in case of death, it should be regarded as a medical, not as a surgical, death, no matter if the patient is operated on late.

Dr. Samuel Lloyd, of New York.—I wish to get on the bandwagon and agree with the statement that has been made in regard to early gastrectomy; and I also want to participate in the unusual condition of throwing bouquets at Dr. Deaver.

It seems to me, the statement he brought out first is the one we want to emphasize and to impress on every internist. We cannot emphasize that point too much, namely, they do not give us these cases early enough. But there is one other difficulty. There is a tradition, a superstition perhaps, among some of my surgical friends, that a case of palpable tumor of the stomach has gone beyond the possibility of operation, and that is being disseminated by teaching and by writing among a certain class

of medical men. Those of us who do and have done considerable surgery of the stomach know very well that there is a large group of movable, localized, large tumors of the stomach, in which, it is true, a large portion has to be removed, as in a case I had the other day, where I took out all of the stomach excepting two inches over the body of the tumor. These cases can be operated on successfully and they give us excellent results.

The statistics of 12 to 15 per cent. of recoveries emphasizes to my mind another phase of the cancer question. Dr. Haggard has alluded to the statistics of early hysterectomies. I think we ought to go back a little farther. Most of us can remember when the statement was made that cancer of the breast was incurable. Now, we know, according to recent statistics of carcinoma of the breast, by the complete operation of Dr. Halstead, of this city (Baltimore), the cures have been increased from 0 to almost 50 per cent. These are the statistics of a single quarter of a century or less. If that is true in cancer of the breast by complete operative treatment, so, too, may we look for great improvement in our statistics with regard to carcinoma of the uterus. We may expect, furthermore, the same improvement from malignant disease of the stomach, when the patients are referred to surgeons early enough, so as to enable them to make complete extirpation of the growth.

Dr. Henry O. Marcy, of Boston.—I could not help thinking, when I was listening to Dr. Deaver's extremely interesting and valuable paper, of another meeting like this, held many years ago. I operated on my first case of resection of the pylorus for cancer in 1876. I read a report of the case before an audience of medical men, and a distinguished physician of Massachussetts arose and said that he had hoped he had heard the limit, before Dr. Marcy spoke, of the audacity of surgeons. He believed there were two organs which were yet untrammeled by the surgeon—the heart and brain, and further, he said, he had supposed, until this meeting, that the stomach was immune.

Dr. Deaver (closing).—I wish to thank the gentlemen for what they have said concerning my paper. We should strive to impress upon medical men the importance of not keeping their patients too long on medical treatment, such as washing out their stomachs and giving them subnitrate of bismuth and having them *x*-rayed, but instruct them to urge their patients to have their bellies opened.

# CYSTS OF THE OMENTUM.

By R. E. Fort, M.D.,
*Nashville.*

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A female, aged two and a half years, was presented February 21, 1906. Was fairly well nourished and gave history of no illness except two attacks of acute indigestion, each lasting several days. Abdominal enlargement was noticed eighteen months before by mother. This had progressed until the child had abdominal circumference of twenty-eight inches at umbilicus. General health and nourishment had been very good, with no symptoms referable to the abdominal condition except some dyspnea upon exercise.

Examination revealed the following physical signs: great abdominal distention, abdominal veins prominent, complete dulness upon percussion over the entire abdomen. Not a resonant note could be found anywhere. Fluctuation and a decided thrill wave could be elicited at any portion of the abdomen. Pulse 100, temperature and urine normal. A reserved diagnosis of ascites or ovarian cyst was made.

The enormous abdominal distention led me to the conclusion that gradual relief of the hydrostatic pressure upon the splanchnic vessels offered the safer course. Consequently, on February 23, five pints of dark bloody fluid were removed by aspiration. In five days examination showed some decrease in the size of the abdomen from the time of the aspiration.

The abdomen was opened through a median incision,

extending from above the umbilicus upward. There presented at once a dark glistening tumor, having the appearance of a distended gangrenous intestine, which owing to its flaccid condition was delivered through the incision without rupture. It was found to be a collection of fluid in the folds of the great omentum, extending from under the pylorus on the right along the greater curvature of the stomach, to and including the folds of the gastrosplenic omentum. A chain ligature of catgut was applied from right to left, including the omentum and the gastrosplenic ligament. There were no adhesions, so the work was rapidly accomplished. The abdomen was closed and the child left the hospital well in two weeks.

Closer observations of the specimen show the absence of a distinct capsule, the fluid being between the folds of the omentum. There was an absence of fat in the omentum, but the vascularity of the omental wall, as would be expected, was great. Closer inspection showed the stab of the aspirator, five days before, had not closed but continued to leak, thus accounting for the decrease in the abdominal measurements. From the day of aspiration to the day of operation there was, however, no free fluid in the peritoneal cavity, the absorption being quite in proportion to the leakage.

The fluid had a specific gravity of 1007, contained albumin and many degenerated blood cells.

In reviewing this subject we find it first described by Gairdeur[1] in 1851. Subsequently, I have collected 21 cases. The case which I have reported, so far as my investigations have gone, will make 22 cases.

The three points which impressed me most are: First, the impossibility of diagnosis without exploration; second, the condition is seen most often in children under ten years (50 per cent. under ten, 65 per cent. under twenty), which leads us to the belief that it is of congenital origin; and third, it occurs more frequently in females, 75 per cent.

Jacobi[2] states that perhaps all of these tumors are of

lymphatic origin and result either from dilatation of the lymph vessels or from cystic degeneration of the lymph bodies. He submits this as Rokitansky's idea, however, and offers no further evidence. The histological characteristics of their encapsulation and the contents of these cysts, however, are of such a variety that it is hard to believe that a distinct etiological factor can be arrived at. Lymph, chyle, serum, both with and without blood cells, have all been reported. Two dermoid cysts have also been reported, Waldy[3] and Spencer Wells[4] reporting such cases.

The presence of a distinct capsule within the folds of the great omentum has been observed, but in a majority of cases we find the tumor separating the folds of the great omentum with no other encapsulation. Again, we find the tumor with a distinct capsule, not within but attached to the omentum by thin ribbon-like bands. This leads Phillips[5] to suggest that they find their origin in the ovary, subsequently becoming detached by twisting of the pedicle, and continuing life by attaching itself to the omentum. The same question presents itself to Jessett,[6] but he admits his position is not tenable by stating that the large vessels passing around the entire cyst came direct from the omental vessels.

Jacobi[7] believes that it will always be a question whether the hydatid omental cyst originates in the liver or in the omentum.

Marsh and Monserrat[8] report a case of a child, less than two years old, with a large cyst with a distinct capsule, showing externally an endothelial covering and covered internally by a coat of fine connective tissue with numerous blood channels, the connective-tissue coat being represented by a marked layer of fibrous tissue. There was great vascularity of the cyst wall, which he suggests as being evidence of the traumatic or inflammatory origin of the condition, but this case had been repeatedly aspirated, and I assume that trauma and local peritonitis, from aspiration, could have produced the same condition.

The case reported by Hearn[9] furnishes the most convincing proof of the congenital origin of these neoplasms. This was a case of a boy, aged eight years, in which he obtained a distinct history of a fluctuating tumor at birth, which decreased in size for some time, after which distention became apparent. It was aspirated at the age of four years, but refilled and was removed four years later by Hearn.

Cotman[10] reports a most interesting case of a young lady, aged twenty-one years, who received an injury by being thrown violently against the shaft of a cart, upon whom he operated three months later. The omentum was curled up under the posterior portion of the greater curvature of the stomach, with a cyst connecting with a perforating wound of the posterior portion of its pyloric end.

This is the only case reported which has a distinct history of trauma, and the symptomatology in this case is also unlike the remaining cases. There were repeated attacks of collapse, vomiting, continued pain, and altogether a clinical picture much graver than we find in the other reports.

The youngest reported case is the one reported by Schramm,[11] a child aged one year. This patient's abdomen measured thirty and one-half inches in circumference. A dark grumous fluid was within the folds of the great omentum.

A case reported by Young,[12] of London, in physical characteristics is identical with the case which I have just reported.

Symptomatology. It is rare that these tumors are discovered in the earlier stages of their development. Pain is not usually an accompaniment, and when it is present it is not severe in character and is usually attributable to digestive disturbances, though several observers have noticed that the radiation of pain was usually toward the liver. It has also been observed that during pain there are marked digestive symptoms—anorexia, dyspepsia, vomiting, diarrhea alternating with constipation, and even cachexia.

The costal type of respiration and severe dyspnea have

also been observed. Hahn states that the special symptoms of tumor of the omentum present an extraordinary analogy to those of movable kidney. I can see that this may be true in those cases in which we have a disturbing symptomatology, but in my own case, as well as in others, there was an absence of subjective symptoms sufficient to point to any diagnosis. It is evident, therefore, that omental tumors present no characteristic symptoms, and the clinical picture is that which accompanies all other forms of cystic abdominal growths.

This is emphasized by the fact that the omentum possesses no physiological function other than a covering membrane. Therefore, it is most natural that small tumors will produce no symptomatology, and that symptoms will only be produced when the weight of the tumor produces dragging or when pressure symptoms occur.

Diagnosis. No case has yet been reported in which the diagnosis has been positively and accurately made. The condition has been diagnosticated as ascites, lipoma, aortic aneurysm, hydatic cyst of the liver, pancreatic cyst, cyst of the urachus, tuberculous and encysted peritonitis.

Péan mentions three points as pathognomic of omental tumor. First, superficial location; second, abnormal passive mobility with downward limitations; and third, absence of functional disturbance; to which Anganeur adds that respiration has little or no effect on the position of the tumor, and Witzel has noticed movement of the tumor with intestinal peristalsis.

I conceive that in the earlier stages of this condition where there has been an absence of an inflammatory process, consequently an absence of adhesions to the adjacent viscera, that these points may be of diagnostic value, but the rare occasions on which we will see these tumors in this stage, I fear, gives but little practical value to their suggestions. This is proven by the fact that Péan, as well as others, had failed to make the diagnosis.

Palpation. Palpation reveals an elastic growth with fluctuation. The thrill wave is present and dulness is absolute over entire tumor on percussion. This shows its cystic nature and that it is anterior to the hollow viscera. Lipoma is the only solid tumor with which it may be confounded, fluctuation having been elicited in this condition.

Differential Diagnosis. The superficial location may simulate neoplasm of the abdominal wall, lipoma, though lipoma is usually fixed. It occurs almost always in adolescence and there is an absence of pressure symptoms. Pancreatic tumors are usually malignant, productive of constant pain, usually produce icterus, fatty stools, and rarely occur before middle life, and as a rule, coils of the intestine yielding a tympanic note may be found over the tumor. Cyst of the urachus, though rarer than omental cyst, if seen early, its origin may be observed to be lower and its mobility is less marked and the same in all directions.

In cyst of the mesentery we usually have a coil of intestine anterior. Ovarian cyst in its earlier stages may be differentiated by pelvic examination and by the physical signs showing that the tumor springs from the lower zone of the abdomen.

Bibliography.

1. Gairdeur. Trans. Path. Soc., London, 1851–52.
2 A. Jacobi. Trans. Assoc. Amer. Phys., 1901, xvi, 232.
3. J. Waldy. Lancet, London, 1889, ii, 642.
4. S. Wells. Brit. Med. Jour., 1890, i, 1361.
5. J. Phillips. King's Coll. Hosp. Rep., London, 1896–97, iv, 101
6. F. B. Jessett. Brit. Gyn. Jour., 1896–97, xii, 156.
7. A. Jacobi. Trans. Assoc. Amer. Phys., 1901, xvi, 232.
8. Marsh and Monserrat. Brit. Med. Jour., 1901, i, 511.
9. Hearn. Amer. Surg. Assoc., June, 1897.
10. H. H. Cotman. Brit. Med. Jour., 1902, i, 1267.
11. H. Schramm. Zentralbl. f. Chir., 1903, xxx, 564.
12. W. McG. Young. Lancet, London, 1905, i, 156.

## DISCUSSION.

Dr. J. Wesley Bovée, of Washington, D. C.—These cases of omental cysts occur so rarely that our experience is limited in dealing with them. I believe that the larger number of omental cysts are inflammatory in origin, and are, therefore, not congenital. A considerable number of cases of these inflammatory cysts are not reported, and they do not attain as large size as the congenital cysts. Therefore, they are not considered of so much moment and do not reach us through the literature.

As to the diagnosis, a particular point to be remembered is that an omental cyst tends to develop downward. We are able to elicit tympanitic resonance below it; while an ovarian cyst develops from below upward. An omental cyst can be pushed upward easily, and not downward; while an ovarian cyst, when small, has a tendency in the other direction. One of the conditions, I would imagine, this growth would be more or less difficult to differentiate from, many times, is hydronephrosis; but I think in that case we would have one element to assist us in the diagnosis, which would be not only examination of the urinary tract, but the resonance probably of the colon on the inner side of the kidney and on the outer side of the omental cyst.

As has been stated, a number of these cysts are probably of hemorrhagic origin. The essayist told us that in his cases the fluid was dark colored, and there were, I think he said, degenerated blood corpuscles.

Dr. Fort.—Yes; degenerated blood corpuscles.

Dr. Bovée.—They may arise from traumatism or from a congenital condition. The cyst may develop from an infection, such as we get from hemorrhages, conveyed through the umbilical cord.

I think the subject is quite interesting, and I am very thankful to Dr. Fort for calling our attention to it.

Dr. J. Wesley Long, of Greensboro, N. C.—The rarity of this condition induces me to say that one of our North Carolina physicians, Dr. J. W. White, Wilkesboro, recently removed a tumor of the omentum, weighing five to six pounds. The patient was a girl seventeen years old, and the tumor was known to have existed since she was two years of age.

Dr. C. E. Caldwell, of Cincinnati, Ohio.—I wish to report a case of cyst of the omentum that I operated on a little over a

year ago. The patient was a colored woman, who was admitted to the Cincinnati Hospital. She came in with what we thought to be symptoms of tuberculous peritonitis; she ran a high temperature curve; she had a large, fluctuating tumor presenting in the whole lower half of the abdomen, and being satisfied it was a cystic tumor I operated, and much to my astonishment, in cutting through the abdominal parietes, found a tumor which could not be separated from the anterior abdominal wall. In other words, the incision went directly through the thickened omentum into the cavity of the cyst which, to my surprise, I found perfectly empty. The cavity was so large that I passed my hand up around and in, everywhere attempting to find the limitations of the cyst. I passed my hand up as far as the kidney sulcus on the left side, also up as far as the kidney sulcus on the right side, passed it down to the symphysis, and could find absolutely no limitation to this enormous cyst. At the bottom of it was a slight accumulation of seroserous looking fluid, which I removed with mop-stick. This fluid smelled like urine, and I thought I had a cyst of the urachus. In fact, that was the diagnosis made. The patient recovered from the operation, and after lying in bed for a number of days began to pass fecal matter through the abdominal opening, and for a number of weeks this was the only way in which she passed feces. Her condition grew better, and for a number of weeks I supposed we might eventually have contraction of the cyst cavity. She improved in appetite. I had her removed from the ward out into the corridor. She increased in weight; she did well, and suddenly the fecal extravasation through the cyst wall ceased and she began to pass feces *per vias naturales*. I did not know what to think about this case, and I do not know yet in a way; still, I think, this cyst being possibly of inflammatory origin became adherent to the bowel; that there had been an ulceration through the bowel, and the opening had closed. To make a long story short, after two or three months the patient began to grow worse and died. The case came to autopsy, and opinion at the autopsy table was divided, but I am rather inclined to think it was one of those cases of tuberculosis of the omentum, with an omental cyst of tuberculous origin. There was nothing characteristic enough in the abdomen for us to say exactly what is was.

I report this case because it has been a matter of considerable conjecture on my part as to what I had to deal with. To be sure, it was not a cyst of the urachus, as it did not communicate with

the bladder. I passed a catheter into the bladder and endeavored to find some connection between the bladder and cyst cavity, but found none. What did I have?

Dr. Fort (closing).—I have very little to add except to remark that my paper has succeeded in bringing out a report of three unreported cases of cyst of the omentum, and for that reason, if for no other, I am glad I brought the subject before the Society. We should report all of these cases because of their rare occurrence.

# VAGINAL SECTION AS AN OPERATION OF CHOICE.

By Henry T. Byford, M.D.,
*Chicago.*

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The principal reason for the performance of vaginal section for diseased conditions of the uterus and appendages has heretofore been the great dangers attaching to abdominal section. It was discovered about twenty years ago, in Switzerland and Germany, that the uterus could be removed per vaginam with much greater safety than per ventrum. About the same time it was demonstrated in this country that the ovaries could, in properly selected cases, be more safely removed in the same way. Not long afterward it was discovered in France that the uterus and adnexa could also be removed, in bad cases of pyosalpinx, more safely by way of the vagina.

But these facts had hardly been established before the statistics of abdominal section began to improve so rapidly that there is today but little difference in the relative mortality. The improved antiseptic technique and the greater facility for dealing with the pathological conditions offsets the greater exposure of the peritoneal cavity, which becomes of less moment than the almost insurmountable difficulties and delays sometimes connected with vaginal section. And as the technique of the latter method requires special training and much practise for its mastery, the abdominal is naturally the operation of choice among general surgeons. However, the ultimate rule of practice must be the selection of the

method of operation that secures the best results to the patient, both with regard to mortality and morbidity.

The cases adapted to vaginal section are those in which the parts affected are more accessible from below, viz., those connected with prolapse or retroversion of the uterus, or with prolapse of the ovaries, or with adhesions low down in the pelvic cavity, and cases of hysterectomy or myomectomy for small fibroids, as well as hysterectomy for other small neoplasms and malignant disease of the uterus. The removal of a fibroid uterus larger than a fetal head at the sixth month is apt to be a prolonged operation and involve such a loss of blood and such depression from the long-continued anesthesia that it may be considered more dangerous than a quickly performed abdominal section. Ovarian cystomas too large to be pushed down into the pelvis should not as a rule be attacked per vaginam, because the danger from an unmanageable complication is greater than that which belongs to abdominal ovariotomy.

In those cases in which the uterus is not to be removed the peritoneal cavity may be opened by an incision through the posterior fornix into the cul-de-sac of Douglas or through the anterior fornix between the bladder and uterus. When the work can be done equally well by either route the posterior incision is preferable, as being simpler and easier. This posterior route is nearly always the better one to employ when the uterus is retroverted, or readily retrovertable, and the cervix can be drawn well down near the vulva. The appendages, as well as small tumors low down on the posterior uterine surface, can be drawn into view and near to the vaginal entrance, and removed or treated as may be advisable, and the incision be completely closed with three or four catgut sutures.

The anterior incision has the advantage of enabling the operator to deliver the fundus into the vaginal entrance and also to draw the ovaries and round ligaments forward within easy reach. Among its disadvantages are a complicated

wound which cannot always be as satisfactorily closed as the posterior median incision, and the presence of the fundus uteri in the vagina to interfere with our manipulations and perhaps be more or less injured. The fundus of course is not always drawn into the vagina, but in dealing with diseased appendages this is about its only advantage over posterior colpotomy. Another disadvantage is that the statistics are not as good for operations upon the ovaries as by the way of the cul-de-sac of Douglas. For shortening the round ligaments and anterior fixation of the uterus, and the removal of small subserous myoma from the anterior uterine wall, the anterior incision is, however, the one of choice.

There are sometimes special reasons why an abdominal incision should be avoided. In such cases the field of vaginal section may be extended by removing the uterus; or, if it be desirable to preserve the uterus, by a lateral colpotomy in connection with an anterior or posterior colpotomy, or with both. By extending the anterior or posterior vaginal incision, whichever has been made, around one or both of the lateral fornices, or connecting a posterior with an anterior incision, we can, by ligating and severing the uterine artery or by skirting or shaving the edge of the cervix with scissors or a spud, separate the broad and sacro-uterine ligaments from the cervix. This not only gives space for the introduction of the whole hand into the peritoneal cavity but enables us to employ intraperitoneal illumination. Or by extending the anterior incision around both lateral fornices and separating the bases of both broad ligaments, as well as the sacro-uterine from the cervix, we can get about as much room as if the uterus were removed. In fact I have extended the vaginal incision completely around the cervix, as for vaginal hysterectomy, separating the bases of both broad as well as the sacro-uterine ligaments and the bladder, and afterwards sutured the parts back in their original relationship without any bad results. Through the opening thus made the torn or perforated rectum can be successfully sutured. Thus

we may say that all sorts of operating can be done through the vagina.

When, however, the cervix is held up and back near the sacrum and adhesions can be felt by bimanual examination to extend high up in the pelvis, or when there is some other condition in the abdominal cavity that requires attention, or when there are tumors that extend high above the pubes, and when the vagina is poorly developed or has undergone considerable senile atrophy, the abdominal route is preferable even though a vaginal section be possible.

The personal equation must also have some weight in the choice of methods. One should do that which he can do best and avoid that for which he is not trained. The gynecologist who has had an extended experience in vaginal section and who has learned to diagnosticate pathological conditions within the pelvic cavity with considerable accuracy, naturally knows how to do things from below and whether they can be done with certainty and celerity. He rightly chooses the vaginal route in many cases. But a general surgeon who has had but little training and experience in vaginal work will more often choose the abdominal route, and in so doing will give the patient a better chance.

Unfortunately some patients still have a great fear of the abdominal incision and will insist that the operation be done by way of the vagina. It is needless to say that the operator should not allow her to force this method upon him unless his experience is such that he knows that he can thereby give her as good a chance for recovery.

With regard to the fate of vaginals ection: While its utility is diminishing, and will undoubtedly diminish with the perfection of the technique of abdominal section for pelvic conditions, there will always be a place for it, and the time has not come when a special training in gynecological surgery can be considered as superfluous. In other words, the gynecologist cannot yet be deprived of the right to live a little longer. But as methods become simplified and indica-

tions better defined, special technique will be of correspondingly less importance.

In conclusion I will venture to make the general statement that vaginal section will retain its place for the treatment of cases that can be completed satisfactorily and quickly by it, and for those in which, on account of inaccessibility or special contra-indications, abdominal section is contra-indicated.

## DISCUSSION.

Dr. Charles P. Noble, of Philadelphia.—It seems to me Dr. Byford has taken a position which those who do not class themselves as adherents of the vaginal route can take as little exception to as possible. I believe the personal equation enters largely into the question of whether we will operate from above or below in many cases for many diseases. It depends on a man's training very largely. So far as my personal efforts are concerned, they have been restricted largely to the abdominal route. But at different times I have tried to have an open mind; I have seen vaginal operators work, and have sought to improve my methods; but it has seemed to me they have needlessly increased their difficulties by their choice of route; and while I greatly admire the skill with which they have overcome difficulties by the vaginal route, it has seemed to me it would have been easier to avoid difficulty by doing abdominal section. I saw a vaginal operator work some time ago, who is an enthusiast for the vaginal route, and what I saw was this: The patient's purpose in being operated on was to have sterility cured. This was the second operation. The first operation was an anterior colpotomy, and a small tumor was removed. After the operation she had a localized peritonitis; but recovered with adhesions. Some years later she came back for the operation I witnessed. The anterior colpotomy was repeated, the adhesions were separated, and in separating them, with some difficulty, one tube was injured. The difficulty in arresting hemorrhage from two adhesions was such that it was necessary to make a posterior colpotomy and put in gauze. In other words, in order to get out of the difficulties present it was necessary to recreate adhesions to save the patient's life. When I have been present and witnessed operations done through the vagina some such difficulties as I have mentioned have always

presented themselves, so I have never felt converted to the vaginal route.

There are certain conditions in which the vaginal route is distinctly preferable to the abdominal. One is for the removal of small fibroid tumors. In fact, in women who have roomy vaginæ, the abdominal operator who would prefer the abdominal route in such cases as that would be unreasonable; also in the removal of fibroid tumors from the interior of the uterus (submucous) the vaginal route is preferable; but with the exception of those two classes of cases, it is my judgment that the abdominal route is far preferable.

Dr. J. Wesley Bovée, of Washington, D. C.—I wish to go a little farther than either of the gentlemen. I think both are a little conservative. One class of cases in which the vaginal route is preëminently applicable is that of accumulations of pus, as pelvic abscesses, in which there is an acute infection, particularly a pelvic abscess where we have infection present or not; that is, whether the microörganisms have lost their virility or not. Vaginal incision and drainage is the proper treatment in those, unless we can prove conclusively they are of tuberculous origin. In many of these cases lives can be saved by that procedure, and even if we do not cure them, we put them in a better condition for a radical and curative operation later.

As to the removal of fibroids, we can remove fibroids from the posterior wall of the uterus as easily as from the anterior wall by vaginal section. We can turn the fundus down toward the anterior fornix of the vagina and remove fibroids from the posterior wall. It is easier to remove these fibroids vaginally than to do it by the abdominal route; and certainly the shock and other incidents of the operation are much less notable than by the abdominal route.

I am sorry Dr. Noble has been unfortunate in his observations of the work done by the vaginal route in the matter of separation of adhesions, doing work on the ovaries and tubes, or shortening the ligaments. I myself am very much inclined to the adoption of the vaginal route in a larger proportion of cases as my work progresses. I would not take an extreme position, such as Dührssen and others are doing, by making an episiotomy or resorting to paravaginal section, as they call it, for the sake of getting more working space by the vaginal route. That is going to the opposite extreme. If the vagina is too small then I prefer the abdominal route. I would not think of the vaginal

route in operating for malignant disease of the uterus, even though it is the body of the uterus that is involved. I would do the work by the abdominal route.

Dr. Edward J. Ill, of Newark, N. J.—It seems to me there is a limitation to the exclusive use of the vaginal route as well as to the abdominal. If we are careful in selecting our cases, we will find there are cases which we can attack to better advantage by the vaginal route than by the abdominal and *vice versa*.

I agree with Dr. Noble that we have a better view of the conditions from above. I have seen operators at home and abroad, who have adopted the vaginal route exclusively, make considerable trouble and annoyance for themselves; I have seen them open the abdomen afterward. There are certain cases, as Dr. Bovée has said, we had better start at from below, such as tubo-ovarian abscesses; and when we operate on these vaginally, in patients who are septic, they can be discharged in a week or ten days, and later get entirely well subjectively. But when we must remove the whole uterus with adnexa we had better do so by the vaginal route. If we must remove a uterus which is large and adherent and cannot be brought down, and feel that the cervix and all ought to be taken away, then we had better begin from below, ligate the uterine arteries, and finish the operation from above. We can do it better; it is an easier operation, and it is easier for the patient.

Dr. Noble.—May I ask those gentlemen who spoke whether they meant celiotomy with opening of the peritoneal cavity or whether they had reference to vaginal incision to reach a localized abscess.

Dr. Bovée.—The pelvic abscess I spoke of forms within the peritoneal cavity, but it is walled off, with the intestines and omentum above. It is a class of cases in which you expose the general peritoneal cavity if you attack them through the abdomen. In attacking them from below you do not.

Dr. John B. Deaver, of Philadelphia.—Personally, I am an advocate of the abdominal route, with few exceptions. Dr. Byford's paper impressed me forcibly with regard to how to deal with certain cases of the type he mentioned.

As to the question raised by Dr. Noble and Dr. Bovée, I agree with Dr. Noble that in dealing with circumscribed abscesses, for instance, through the abdomen we should not open into the peritoneal cavity. Take extra-appendiceal abscesses, we open them and do not open into the peritoneal cavity. I should not

call that a vaginal operation. I rose particularly to say, that in my visit abroad I saw a celebrated vaginal operator work, but he did not do a single operation by the vaginal route. All of his operations which I saw were done through the abdomen.

Dr. J. Mason Hundley, of Baltimore.—I was in Vienna two weeks this summer, and during that time saw a great deal of vaginal work done. I saw operators remove fibroids weighing four to six pounds by the vaginal route. One operator I saw does most of his surgical work by the vagina. I only rise to say that they do an enormous amount of surgical work abroad through the vagina.

Dr. Byford (closing).—It was not my intention to include cases in which the peritoneal cavity is not opened; and it was the object of my paper to show the limitations of the vaginal route rather than to advocate it indiscriminately. When I called attention to the fact that large fibroid tumors should not be removed vaginally, it was not because they could not be removed by that route, because they have been. Ovarian tumors weighing thirty pounds have also been successfully removed vaginally, for the large cysts can be evacuated from below and the collapsed walls be drawn into the vagina. We may get along nicely with such cases for a few times, but to keep on is to court disaster. Therefore, I think it is time for some of us who have done so much of our work through the vagina, and have taken an extreme position in regard to it, to point out the dangers.

# PRELIMINARY PAPER ON THE TREATMENT OF THE BONE DEFORMITY OF THE THORAX AND SPINE IN SCOLIOSIS BY PLASTER JACKETS, UTILIZING THE EXPANSION OF THE LUNGS AS A CORRECTIVE FORCE.

By Michael Hoke, M.D.,
*Atlanta, Ga.*

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In November, 1903, I reported[1] the case of a young lady upon whose ribs I had operated in order to correct a very bad deformity of the chest—the result of scoliosis. Previous to the operations she had gymnastic treatment for a year and a half. This treatment had made her more flexible, but the deformity of the bones was as pronounced at the end of the treatment as it was when the treatment was begun. A number of attempts were made to conceal the deformity without success. At that time there was no known method by which the degree of bone deformity which she had could be corrected. The operations were, therefore, devised and performed. The details of the operations will be found in the reference.[2] Considering all the accompanying circumstances I may say the result is satisfactory. The patient presents no appearance of deformity with her clothes on without the use of pads, but she wears a celluloid corset. Her spine is still as crooked as it was when the operations were done, but the lines of the thorax from the armpit down

[1] American Journal of Orthopedic Surgery, November, 1903.

[2] A Study of a Case of Lateral Curvature of the Spine. A Report on an Operation for the Deformity.

are almost symmetrical. There is no prominence of either hip, the shoulders are in the same plane with the pelvis, and neither shoulder-blade protrudes. Her circumstances were such that she has at no time been able to remain in the city for postoperative exercises and massage. I may assert with accuracy, that at the time these operations were done upon this patient it was impossible to accomplish the result obtained by any other method of treatment than the operation performed.

It occurred to me after the operations upon this patient, when she returned for a jacket, that the expansive power of the lungs might be used in these cases as a corrective force to fill out the flat areas while the prominences of the chest were being prevented from expanding, hence the purpose was formed to try this experiment upon some cases as badly deformed as the case operated upon in order to see if a similar result might be obtained by this simpler method. Thus no other cases were operated upon. The three cases herein reported show that better results may be accomplished than by operating. While there remains a great deal of detail work to be done, the progress made so far in this method warrants this preliminary report.

Fig. 1 shows how symmetrical is the normal thoracic element. It is evident that in the growth and development in life of the thorax of which the element was a part, equal forces were at work at corresponding areas symmetrically expanding the elements. These forces were the respiratory forces of expansion. Given normal lung tissue in both lungs, man's natural posture, and no pressure upon the chest wall, then corresponding areas of the chest wall are subject to the same amount of expansive lung force eighteen times per minute. The bones of each half, therefore, grow alike and thus the thorax is composed of two symmetrical halves.

We know that impairment of a portion of lung tissue is always followed by diminished respiratory expansion of the chest over the affected area. If the equal expansion over

corresponding areas be made unequal by disease within the lung or of the pleura it is followed always, as we know by thousands of clinical observations, by flattening of the chest wall over the impaired lung tissue. It is not the disease *per se* that flattens the chest wall; it is the diminution of the normal respiratory expansive force mainly that causes it. Also we know by thousands of clinical observations that if through a period of time a portion of lung expands more than a similar portion of the other lung, the chest wall over the former will enlarge more than the corresponding area of chest wall over the latter—compensatory expansion.

It is not disease alone that causes corresponding portions of the lung to expand unequally. Let a normal individual with naked chest stand in front of you. Lay the hands gently on corresponding areas of the chest and direct the individual to breathe deeply. One feels the equal expansion of the chest beneath the right and the left hands. Press very gently upon the chest with the left hand; the chest wall beneath the right hand is felt to expand much more forcibly than when there was no pressure with the left and the expansion is greater than normal. Try this simple experiment with the hands on any areas. If they be corresponding ones the observation is still correct. If a normal person stands, bends the head and the body to the left so as to assume a right convex scoliosis (viewed from the back) the result upon the expansion of the chest is as follows: The left anterior half (by "anterior half" is meant the area from the midline in front to the mid axillary line by "posterior half," from the spine to the mid axillary line) of the chest expands much more than when standing erect, which we shall call the normal; the right anterior half expands much less than normal; the left posterior half expands less than normal; the right posterior half more than normal. It makes no difference whether the person, preserving the assumed right dorsal scoliotic position, bends in forward flexion at the same time or in hyperextension. There is some difference in the degree of the expansions

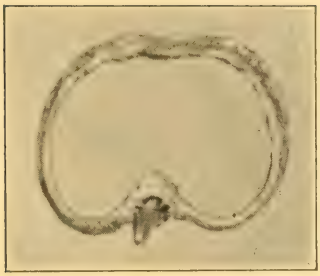

Fig. 1. Photograph of two complete "thoracic elements," the fourth and fifth taken from a cadaver selected at random, the lungs of which were normal. The term "complete thoracic element" is used to represent the bone ring formed by a vertebra, the attached ribs, and the co-extensive part of the sternum.

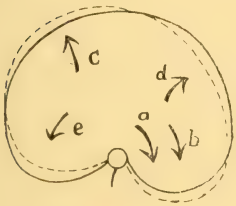


Fig. 2

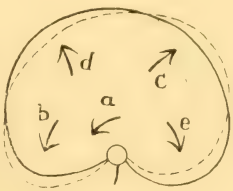


Fig. 7

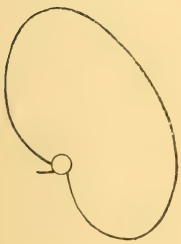

Fig. 6

Fig. 10

Fig. 6.—Notice how this figure corresponds to the dotted lines in Fig. 2.

Fig. 10.—Notice how this corresponds to the dotted lines in Fig. 7. Examination of this patient's respiration showed increased expansion over the upper part of the right anterior and left posterior halves and deceased expansion over the anterior and right posterior halves, not matter what the posture, for no matter what posture she might assume the type of scoliosis was not changed.

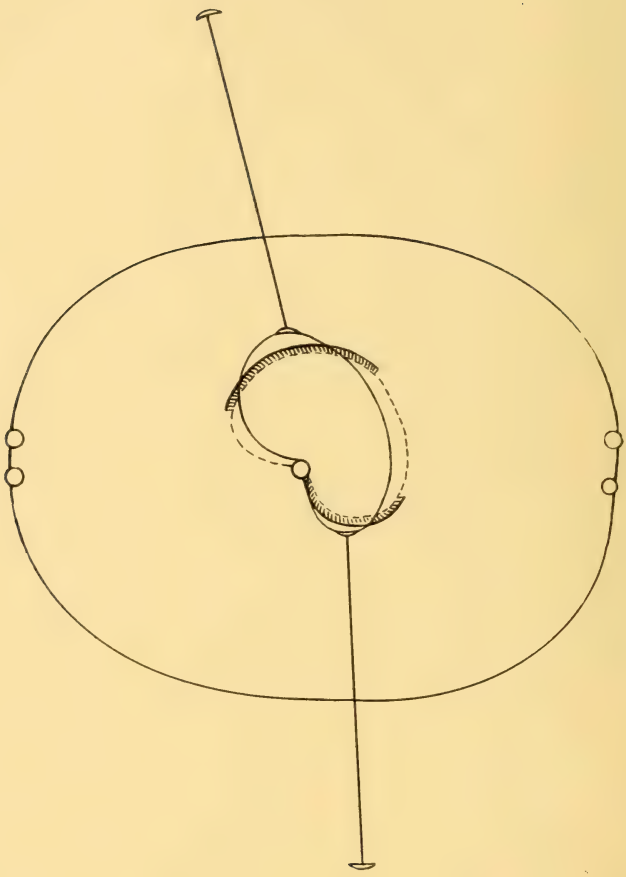

Fig. 25

but this does not affect the principle involved. One of the reasons for the decreased expansion of the right anterior half in the above simple experiment is that by the posture the right intercostal fascia and muscles and abdominal fascia and muscles are put upon the stretch; this makes the right anterior more tense than normal, which is added to if the head be far to the left by the sternocleidomastoid and scaleni pulling upward upon the clavicle and first and second ribs. The chest wall over the front of the right lung becomes somewhat tense and this condition offers opposition to the expansion over the area in question. So easily is the symmetrical expansion disturbed that this slight resistance is sufficient to cause the other side to expand more than the normal degree. The right posterior half expands more than normal because of the lessened expansion in the right anterior half; the left posterior half expands less because of the increased expansion of the left anterior half; the left posterior half expands less because of the increased expansion of the left anterior half. The import of this is better understood by expressing it graphically.

In Fig. 2 let the dark lines represent an element of a normal person. In this element, when the person is erect, the respiratory expansion is symmetrical. Let the person assume a right convex scoliosis with slight forward flexion. There is immediately a rotation, great or less as the case may be, of the vertebra in the direction of the arrow *a*, and a tendency for the whole bone ring, the element, to so rotate. Immediately too, as determined above by feeling the expansion, the element is subjected to an expansive force in the direction of the arrows *b* and *c* greater than ever before, and the expansion is not so great in directions of the arrows *d* and *e*. Thus each time the person breathes the shape of the element is changed slightly to the figure illustrated by the dotted lines. This happens eighteen times per minute to all elements participating, in greater degree to the most flexible.

Upon examination it was found that with each inspiration

the boy's chest over the left anterior half expanded more than the right anterior half, and more over the right posterior half than over the left posterior half.

During the three years the deformity was developing the posture, dependent in this case upon a short leg plus "skeletal insufficiency," caused the respiration after the manner above indicated to force out the chest where the respiration was increased, and the chest to flatten where the respiration was decreased. This agrees with the observations on the shape of parts of the chest influenced by decreased respiration in one part and increased respiration in another part.

If the individual, by bending to the right, assume a left convex scoliotic posture, the result upon the respiratory expansion is the opposite of that produced by the right convex scoliotic position. In this instance the right anterior half of the chest expands more than normal, the left anterior half less than normal, the right posterior half less than normal, the left posterior half more than normal. The effect upon each element then is as illustrated by the dotted line in Fig. 7.

It is plain from the foregoing illustrations and statements of fact that the respiratory expansion forces induced by the scoliotic posture, no matter what may be the original cause of the spine-bending, tend to produce the same type of chest deformity as the rotary force, are in themselves rotary forces, and add to the torsion of the spine, while their predominance in one part of the chest materially assists in forming a prominence of the thoracic wall and in another part their relative weakness permits the chest to flatten. As the shape of the ribs and, therefore, the shape of the thorax is dependent upon the foregoing, it is plain that these forces must be continuously opposed in the treatment of the bone deformity.

Cases I, II, and III, will illustrate the result upon them thus far from acting upon these principles.

Case I has been under treatment by correction jackets since May 1, 1906. Fig. 3 shows a photograph of the back of this patient when treatment was begun May, 1906.

Case II was under treatment by correction jackets for fourteen months. He was a carpenter. Nine months ago he left the city and has not returned. Circumstances prevented his receiving gymnastic treatment subsequent to the use of the jackets. I have written several letters to him but have received no reply.

Case III has been under treatment intermittently for three years. Twice she went for six months without a jacket, several times she has gone for one and two months without a jacket. In order not to have too many illustrations, only these cases of extreme deformity are reported in this article.

The examination of Case II showed diminished respiration over the anterior part of the right lung and posterior part of the left lung, increased respiration over the front part of the left lung and back part of the right lung. Naturally, then, the shape of the chest elements was the same as in Fig. 6,the same as in Case I, for they are the same type of scoliosis. Compare the rib prominence and flatness in the back with similar photographs of Case I. Unfortunately I did not get a photograph of the shape of his chest in front in the beginning; the contour was like that shown in Case I, prominence in the left anterior half, flattened in the right anterior half.

Fig. 8 is a photograph of Case III, taken before treatment was begun, bending forward outlining the prominence of the ribs on the left of the spine. Examine this photograph closely and it will be noticed how small is the transverse diameter of the half of the thorax to the right of the spine. It is noticeable too that the angles of the ribs on the right of the spine have been straightened and obliterated. It is not so plainly shown in the photograph as it was upon the patient.

There is not as much improvement shown here as in the other cases. The treatment has been too intermittent and only recently has appropriate apparatus been devised for the application of the jackets, making the technique accurate for this case. No photograph of this patient lying on the

back was taken in the beginning to show the prominence of the right front half and the flatness of the left front half which would be expected in convex scoliosis from what has been said in the first part of the paper. Fig. 9 taken recently shows, however, that such is still the case.

None of these cases have had any treatment for the correction of the deformity but jackets and apparatus applied on the principles laid down herein.

The interpretation of these plain facts forces upon us certain principles which must be applied in the treatment of all cases of scoliosis where there is any deformity of the chest. It is evident from the foregoing that if a plaster jacket even touches the chest it limits the expansion at that site; if it touches everywhere it limits the expansion of the area it is desired to fill out as well as the area it is desired to keep from expanding. Nothing must touch the area desired to be filled out; not even a gauze shirt. A careful study of the thorax by observation, palpation, and auscultation must be made to find out what parts of the lungs are expanding too little and what parts too much.

The jacket must be put on so that while it is being applied the rotary forces and the deforming respiratory forces are opposed. This cannot be done unless the jacket is applied with the patient in the position seen in Fig. 24.

The weight of the body must be used as a counter-force so that the correction-force, while it is being applied, be not lost in some compensating motion due to the patient's voluntarily giving to the pressure. The patient's legs must be elevated, otherwise he will yield in the lumbar region to the pressure applied to the prominence of the ribs. This elevation of the legs prevents undesirable lordosis. The jacket must not be applied with the patient in the upright position, nor lying face downward on a stretcher. The apparatus shown in Fig. 24 has been found after much experience to be simple and susceptible to use in applying the principles. In a later article the details of its construction will be given.

FIG. 3 FIG. 4 FIG. 5

FIG. 3.—View of a patient's back (right convex scoliosis) while standing. (Case I.)

FIG. 4.—Same patient while bending forward. (Case I.) Notice the rib prominence on the right side of the spine and the depression on the left side of the spine.

FIG. 5.—Same patient lying down on his back. (Case I.) Notice the prominence of the left anterior half and the flatness of the right anterior half of the chest. The elements in this thorax have the shape shown in Fig. 6.

Fig. 8.—Photograph of a patient, left convex scoliosis, shows the prominence of the ribs in the left posterior half and the flatness of the right posterior half. (Case III.)

Fig. 9.—Photograph of the same case lying on her back. At *a*, the right anterior half, the chest is more prominent than the left anterior half. The patient's left costal border was more prominent than the right costal border. The latter fact is more apparent in the photograph. This was recently taken. No photograph of this patient in this position was taken before treatment was begun. (Case III.) An element in this case in the upper part of the chest had the shape shown in Fig. 10.

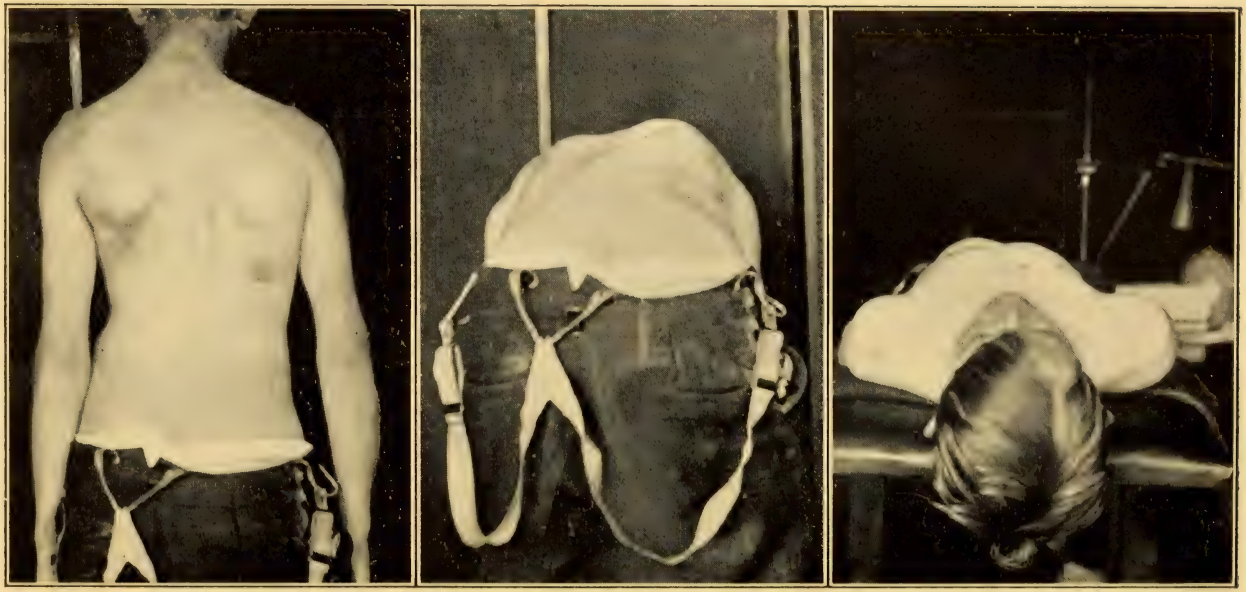

FIG. 11 FIG. 12 FIG. 13

FIG. 11.—Shows the same view as Fig. 3, taken November 26, 1906. Fig. 4, taken with patient bending forward, outlines the prominence of the right of the spine when treatment was begun. (Case I.)

FIG. 12.—Shows the same view as Fig. 4, taken November 26, 1906. The figure is a photograph of the patient taken May, 1906, as he was lying on his back. (Case I.)

FIG. 13.—Shows the same view as Fig. 5, taken November 26, 1906. (Case I.)

FIG. 14 FIG. 15 FIG. 16

FIG. 14.—Shows a view of the back of the patient in the jacket and apparatus. This was taken August, 1906. All the steel parts of the apparatus were detachable. They were worn in the day and removed at night. The plaster jacket, of course, is non-removable. At present a different combination of plaster and apparatus is being worn by this patient. (Case I.)

FIG. 15.—Shows another view of the patient with jacket on. (Case I.)

FIG. 16.—A side view of the patient with jacket on. The combined jacket and apparatus was worn with comfort. Note how the apparatus pushes the thorax beneath the left shoulder forward. Note the very large fenestra. (Case I.)

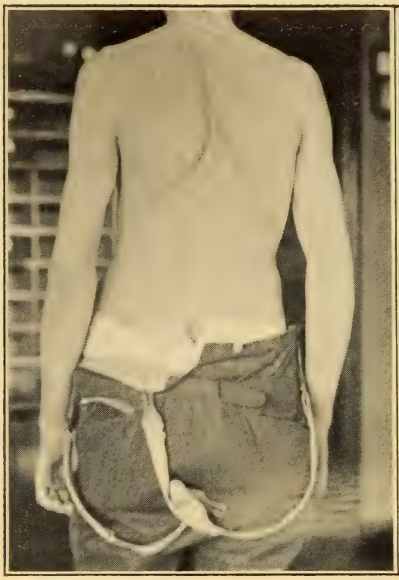

FIG. 17.—Photograph of Case II when treatment was begun.

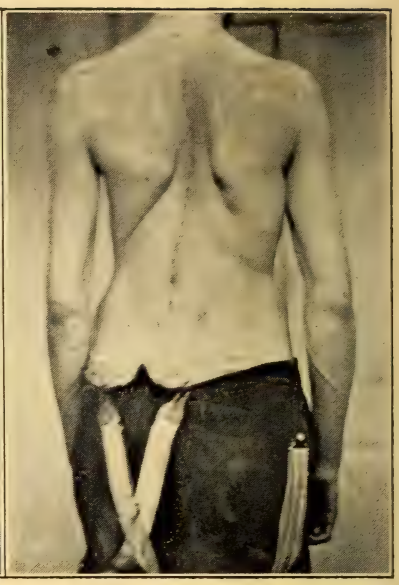

FIG. 18.—A similar photograph taken nine months ago, at the end of fourteen months' treatment. (Case II.)

FIG. 19.—Photograph of Case II bending forward, showing the rotation and prominence of the ribs on the right of the spine before treatment was begun.

FIG. 20.—A similar photograph of Case II taken at the end of fourteen months' treatment.

FIG. 21 FIG. 22 FIG. 23

FIG. 21.—Photograph of Case III taken three years ago.

FIG. 22.—Photograph of Case III similar to Fig. 21 taken recently. She has been without a jacket seven months.

FIG. 23.—Photograph of Case III taken recently, similar to Fig. 8. Examination of this photograph shows that the transverse diameter of the right lung space, of the right posterior half of the chest, has been increased and that the ribs on the right side are regaining their anatomical shape. The angles of the ribs are forming again.

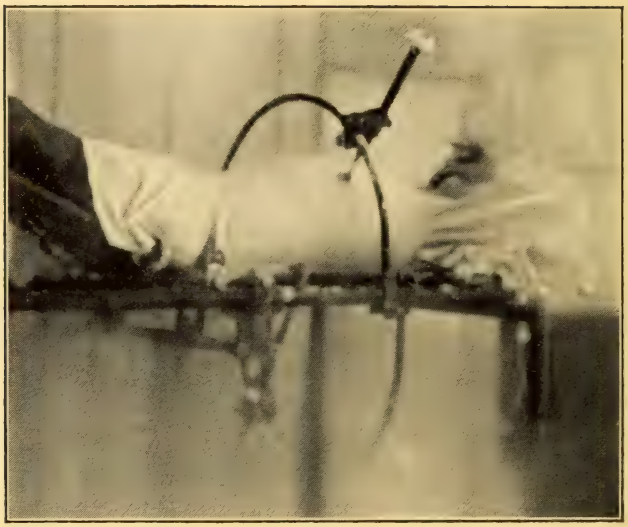

Fig. 24

Suppose Case I—right dorsal scoliosis—to be upon the apparatus; the legs are elevated, the pelvis is clamped, the apparatus applied as is shown in Fig. 24. Fig. 25 illustrates how the corrective force is applied to the elements while the jacket is being applied.

When the pressure is applied the chest elements assume, say, the position of the dotted lines. After the plaster has hardened, the pressure pins are removed. The next day the plaster is cut away from over the areas of the chest it is desired to fill out. Thereafter, so long as the jacket is worn, the respiratory expansion is concentrated upon the flat areas now free to expand, while the plaster over the prominences keeps the lung beneath the prominences from expanding but little. The shaded areas represent the plaster left over the chest prominences in order to prevent expansion of the lungs beneath. The gaps between the shaded areas correspond to the areas of flattened chest wall left uncovered by plaster and beneath which the respiratory forces are compensatorily increased. Steel pieces are incorporated in the plaster, to which are attached any steel attachments it may be found necessary to use to hold the shoulders, head, or the upper exposed parts of the chest. This feature is illustrated by the photographs of Case I.

The requirements for appropriate treatment of these cases taxes our ingenuity to an extreme degree, for no two cases are alike; therefore, one cannot lay down more specifically the details of application of the principles. These details vary and must be worked out in each instance. The cases show great improvement in the shape of the thorax, marked decrease in the rotation, and improvement in the curve of the spine. In a later paper skiagraphs of Case I will be presented.

The result obtained by reversing, under pressure, the method of respiratory expansion in these cases of scoliotic deformity must force one to conclude: That the disturbance of the symmetrical expansion by its dynamic influence upon the

ribs must be a factor in permitting the spine to bend and, therefore, be a constant influence in the rotation of the spine, as well as a most powerful factor in determining the shape of the deformed thorax associated with the scoliosis; that in the treatment of the condition the above principles must be applied in the treatment of the bone deformity.

It is not intended in this article to minimize the other causative factors, anatomical, statical, physical, etc., so long and correctly recognized as causative influences in the production of the deformity. Whatever causes may produce the scoliotic posture, increase the respiratory forces in a part of the chest, diminish them in other parts, as explained above, so that this changed respiration *adds to the rotation*, forces the chest wall into a prominence where the respiration is exaggerated, and permits the chest to flatten where the respiration is diminished. The vicious circle is made complete by this alteration of the respiratory expansion.

## DISCUSSION.

Dr. C. E. Caldwell, of Cincinnati.—I have been extremely interested in this paper of Dr. Hoke, because I have worked along the line of orthopedics for a number of years, and when Dr. Hoke's paper came out (I think it was in the *Journal of the American Orthopedic Association* two or three years ago), I read it not only with great interest but with amazement. I read it with profound admiration for the man who had the courage to undertake so radical an operation as the one undertaken and shown in the pictures that have been exhibited. I had under my care at the time a number of infantile scoliotic cases, and I hesitated for a time as to whether I should take courage from Dr. Hoke's work and attempt to do similar work along these lines, and I said to myself: "Don't be an ass, because you do not know how to do it, and perhaps Dr. Hoke does, and if he is getting such favorable results I will see him do the operation before I attempt it." I waited for further reports of this operation, and not seeing any, I concluded perhaps it was not practicable, because I thought it was a

serious operation, and perhaps it would not altogether meet the indication and might possibly cause death to the patient under certain conditions. If I understand Dr. Hoke correctly, he made the statement that the lung is one of the factors which bring about the condition to which he referred.

Dr. Hoke.—It is a factor, but I do not minimize other factors.

Dr. Caldwell.—This is an *a posteriori* argument, a reasoning from effect to cause rather than from cause to effect. The body develops in somatic segments, and does not develop symmetrically. If you plant a tree where it is not crowded, give it plenty of air and the things necessary for the development of that tree, it will develop symmetrically. Its somatic segments will grow out symmetrically. Now, the newborn child has a symmetrical chest. No doubt the argument is sound that conditions of empyema bring about curvature of the spine; but they do not bring about a condition similar to this; it is not a condition which can be in any way likened to the condition of rotary lateral curvature. There may be an inherent defect in the child; inherent inequality in its nerve distribution. We may call it what we please. We do know, however, that certain children have a tendency, under unhygienic surroundings and with perhaps a bad inheritance, to develop at the age of adolescence, or even younger, scolioses.

Dr. Hoke has shown us a very ingenious plaster method of exerting pressure on one side of the body, leaving the other side open. That does not mean he is changing the respiration of the lungs; he is doing what gardeners often do to trees, straightening them, relieving the pressure on one side and supporting it on the other.

I am under obligations to him for this beautiful demonstration. I do not wish to appear hypercritical, but we are misconceiving the situation, and while Dr. Hoke is undoubtedly getting the results which he describes, still he is attributing them to the wrong process, and I would take exception to the fact that it is by reversal of the respiratory processes, but rather by purely and ingeniously mechanical pressure, with no pressure exerted on the opposite side. I would like to see some of these cases. While photographs are convincing, they are not as thoroughly convincing as the seeing of the patient. When a case of curvature comes to me with this fixation rotation, with great deformity, I look upon it with despair, and wish somebody would take the case.

Dr. Edward H. Ochsner, of Chicago.—I read Dr. Hoke's article, referred to in his paper of today, a few years ago and was convinced that he was then on the wrong track, but I am glad to see that he now takes a different position and to me his present attitude is very plausible. We know that the primary cause in the development of scoliosis is a faulty attitude. This is followed by asymmetrical muscle development and finally by actual bone deformity. I have seen a number of anatomical specimens of extreme cases of scoliosis in which the vertebræ had become greatly deformed, so much so that one side was twice as thick as the other. This fact is apt to make one very pessimistic in reference to the extreme cases of scoliosis, and it is a notable fact that those teachers and authors who know most about scoliosis from the standpoint of the deadhouse have the least faith in any form of treatment in these cases. They have told us that when this bone deformity has once developed all treatment is useless. Personally, I am convinced that this position is wrong. I believe that if we can so brace our patient as to overcome the false attitude and get the weakened muscles to regenerate the vertebræ will again assume their normal shape, provided, of course, this is done before the patient has stopped growing. The tendency toward correcting all deformity in the growing individual is so great that if we give it half a chance things are almost sure to right themselves. This point was forcibly impressed upon me by the following experience: A patient on whom I reduced congenital dislocations of both hips came to autopsy three years later and it was almost impossible to tell that these hips had ever been dislocated. The acetabula had become so markedly deepened, the cotyloid ligaments so well developed, and the capsules so markedly shrunken, that the joints were practically perfect anatomically. From this and other experiences I am led to believe that if we follow the directions given us by Dr. Hoke this morning, these vertebræ will again assume their normal shape and we will actually cure such patients, provided, as stated above, the treatment is instituted before they have stopped growing. Up to the present time the treatment of the more severe cases of scoliosis has been very unsatisfactory. These people go to the orthopedic surgeon or orthopedic institute year after year and if they have a good imagination they can be made to believe that they are really getting better. I have known some of these patients who have been under the most careful treatment for ten years and even then they were no better than when they began the treatment.

Dr. Robert T. Morris, of New York.—I wish to say a few words in regard to one or two points mentioned by Dr. Ochsner. Briefly, when deformity is corrected by mechanical means temporarily, the pressure which is brought to bear upon the deformed bone will result in an absorption of the bone at the point where the greatest pressure is brought to bear, and there will then be a tendency toward symmetrical development. The natural tendency of all disease is toward recovery. That is an old saying. The tendency of somatic segments toward symmetrical development belongs as a sub-head in this classification.

I look upon scoliosis as the result of a functional muscular insufficiency primarily, and that the scoliosis is a symptom; consequently, rotation and curvature are symptoms, just as a cough or a sneeze. Scoliosis, then, is a cough or sneeze. After you have corrected the deformity, as Dr. Hoke does, and given opportunity for symmetrical development, then is the time for the other man to step in to help the orthopedist, who does not call in other men, as a rule, to find out what is causing the muscular insufficiency. Here we enter an interesting field. We should look out for nasal hypertrophies and spurs, and find out whether they are present or not.

We should look up the matter of eye-strain; have the eyes examined carefully by a competent ophthalmologist, so as to be able to rule out certain possible causal factors. I do not say that eye-strain is present in a large proportion of cases; but it is very essential to look into these various things to determine whether they are present or not, and if not, we must rule them out as a moral demand. I know a lot of orthopedists who never listen to the ruling out of eye-strain, to preputial adhesions, etc. There are a number of various causes for this cough or sneeze of the bones, so to speak, which must be looked up and worked out, and it is not regularly done.

Dr. William S. Baer, of Baltimore.—We are greatly indebted to Dr. Hoke for the ideas he has brought forward today in the treatment of scoliosis. Certainly, the expansion of the lungs must play an important part in the treatment of the deformity. We all know what a factor empyema is; and Dr. Morris has referred to obstruction of the nasal passages, etc., as causal factors. We likewise know how enlarged tonsils play a part in the development of lateral curvature.

Most interesting articles have lately been brought out by Drehman, of Breslau, also by Dr. Bohm, of Boston, showing the congenital defects in the spine, which may be etiological

factors in starting these lateral curvatures. Drehman, in his paper entitled "Zur Anatomie der sogenannten Halsrippenskoliose," and Bohm, in his paper on "Segmental Differences in the Cord," show that there are variations which occur at different levels. The ribs on one side of the body may be placed one vertebral body lower than those of the other side. The variations pointed out by Dr. Ochsner throw considerable light on the etiology of these affections in the last year. But we have not paid much attention, it seems to me, in regard to the development from a respiratory standpoint in these cases. Dr. Hoke's article is a very suggestive one. If we consider these congenital defects in the spine, and if we can get at the cases at an early date, and see the difference in shape of the various ribs, and then take into consideration how these defects may alter the respiratory function of any given part of the lung, it seems to me that we shall have made quite a stride forward toward the cure of lateral curvature.

Dr. Hoke (closing).—With reference to the remarks made by Dr. Caldwell, he drew a comparison between individuals and trees. Trees have no lungs. They have their expanding force within the bark, which is not like that in the chest of a human being; such a comparison cannot be made. Yet it is a familiar thing—the influence of external pressure upon the shape of a tree trunk. Physical principles are connected with the shapes of all living and growing things.

As to empyema, I have treated a case in which there was empyema and identically the same thing existed as has been mentioned, as you will see by looking at the photographs here. I think these photographs will convince you that what I have said and pointed out means a great deal in the treatment of this deformity. This change in the respiratory expansion (indicating) is simply one of the factors set in motion by other things, and until you combat this factor, you will not correct the chest deformity. The rotation which exists in these cases can be very largely corrected. If you put a jacket on a child's chest tight, cut out a large area, put your finger between the margins of the jacket and the child's chest, and ask it to breathe—you will feel how it pinches your finger. You will then get a good idea of how strong the respiratory expansion is when it is done in this fashion.

Dr. Morris has said that these patients must be examined for many things, as eye-strain, nasal hypertrophies, etc. In addition to looking for those things, we must study carefully the respira-

tory expansion of each individual case. We have to use the stethoscope and find out which parts are expanding and which are not sufficiently; then we have to apply a jacket suited to the individual case. Every jacket varies as you cut it out. For instance, a variation of one-half inch will keep the lung from expanding. You may theorize all you please with regard to the treatment of these cases, and about other things, but if you do not carry out the treatment along the lines I have suggested, you will not get as good results. Orthopedic men in the past have been paying too much attention to the spine, and not enough attention to the other factors that are concerned in these deformities. It is very manifest that when we have a case with such a serious deformity as this (pointing to chart), it is absolutely impossible to redevelop accurately all the sides of the vertebræ. I do not think it can be done; but if we can correct enough for the patient to have a symmetrical shape, which will hold, there is reason to believe that we will have a practical result, one which is just as perfect as the curing of a man of appendicitis by operation. The object of the treatment in these cases is to try and make the patient lose the appearance of deformity, and I believe this can be accomplished.

# APPENDICITIS IN THE NEGRO.

By Hubert Ashley Royster, A.B., M.D.,
*Raleigh, N. C.*

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Sweeping statements concerning the susceptibility of different races to certain diseases sometimes stand in need of revision.

The accumulated experience of many observers furnishes the profession with new facts which, gathered into tables of statistics and carefully studied, will serve, by inductive reasoning, to establish, to modify, or to alter completely our accepted opinions.

For many years the medical teachers of this country were accustomed to make dogmatic assertions in regard to the rarity of this or that pathological condition in the negro race. Many of us whose work has been largely among the colored people of the South have had to unlearn much of this instruction. To speak more specifically, it was held that ovarian cysts did not occur in the negro woman, and the writer as a student was so taught by one of America's greatest gynecologists a little more than twelve years ago. Following the lead of I. S. Stone, who first looked into the question, it was my privilege to present two communications on the subject and to indicate that an ovarian cyst might be slighted in the diagnosis of doubtful abdominal enlargements in negro women, being mistaken for the more common uterine fibroid. Altogether I have operated on upward of a score of ovarian cysts in the negress. So has it been with cancer, to which the Southern negro certainly possesses no immunity.

As far as I am aware, the subject of appendicitis in the negro has never been very thoroughly investigated. No

reference to it is made in any of the text-books on general surgery and it is mentioned in only one of those treating of the vermiform appendix and its diseases—to which attention will hereinafter be directed. The periodical literature of medicine contains nothing in regard to it.

Indeed, it may be doubted whether the matter is of sufficient importance to demand attention at the hands of the profession generally. My interest was aroused by the occurrence of several consecutive cases recently in the wards of a hospital for negroes of which I have charge, and my determination to study the question was directly incited by a passage from H. A. Kelly's large monograph (*The Vermiform Appendix and Its Diseases*), which was quoted in more or less detail by Dr. A. E. Baker,[1] of Charleston, S. C. This reads as follows (p. 359):

"The negro race, however, seems to be comparatively exempt from the affection. In order to obtain some information upon this point I wrote to several surgeons practising in southern cities having a large proportion of negro inhabitants. The replies were unanimous in regard to the rarity of the disease in this race. S. C. Briggs, of Nashville, Tenn., out of several hundred operations for appendicitis could recall only one upon a negro. He stated, however, that as the colored race in the South have their own physicians, they did not always come under the observations of others; nevertheless, he thought that the disease was rare. H. J. Inge, of Mobile, Ala., out of 149 operations had but 1 in a negro, and by inquiry among other physicians found its occurrence in the race to be equally rare in their experience. G. H. Noble, of Atlanta, Ga., had never operated upon a negro, for appendicitis. L. L. Hill, of Montgomery, Ala., ('in the black belt'), with a colored population of 9000, could collect the histories of only 4 cases that had occurred among them, and found that the physician who had been consultant for a number of years at Booker Washington's

[1] Journal of the South Carolina Medical Association, January 21, 1906.

school, where there are about 1400 colored students, did not remember to have ever seen a case there; at the Alabama Polytechnic Institute, however, twenty miles distant, where there are 400 boys, there was an annual average of 8 cases. . . . There are 800 negro prisoners at one of the Alabama coal mines, and the physician in charge states that appendicitis is unknown among them. . . . The statistics of the Johns Hopkins Hospital show, that while the number of admissions of colored to white averages about 1 to 4, the number of cases operated upon for appendicitis is as 1 to 12." (Dr. J. C. Bloodgood tells me that he has just looked up this matter and he finds no difference between the two races as admitted to this Hospital in their liability to appendicitis.)

To recount merely my own experience would in no wise settle the issue here raised. It will do something, however, toward initiating the inquiry, and coupled, as it will be, with a number of personal communications, may furnish the ground for some deductions. During the ten years in which I have practised, I have removed the diseased appendix from negro patients 54 times. In 40 of the cases, a pathological appendix was found while operating for other conditions—pus tubes, fibroids, etc. Besides a few of these which contained pus, over half of the others were filled with fecal matter, usually soft. Practically none of these cases presented a definite history of appendicitis, but only that of the original lesion. Fourteen cases of primary appendicitis in the negro were operated upon, 3 in the first seven and a half years and 11 in the past two and a half years. During the latter period, two and a half years, the total number of appendix operations on both whites and negroes was 124, giving a relative frequency of practically 9 per cent. in the colored race. The white population outnumbers the black in every locality from which these patients come. There was in the series of 11 about an equal division between the pure negro (or rather the dark negro—because the pure ones

are few and far between) and the mulatto. One of the last named had gall-stones associated with appendicitis. In all save three the appendicular disease had advanced to pus-formation before being operated upon. One of these was an early acute case, the only one which occurred entirely under my own observation, and the other two interval ones.

For the purpose of adding substantially to these statistics of mine, and of determining whether like observations had been made by others working in similar fields, I wrote letters to many prominent operators in the South, asking them these questions:

1. How many cases of appendicitis in the negro have you seen?

2. What is your opinion as to the relative frequency of this disease in the colored race?

To fifty letters sent out there were thirty-five replies, a review of which would be pertinent to our subject. Six of these are difficult of classification, because either of incomplete records or limited observation. With one exception (Nicolson, who is sure the disease is very common among negroes, more than is supposed), however, this sextette of observers consider appendicitis in the negro quite infrequent "in comparison with its widespread prevalence among Caucasians" (McMurtry, Finney, Ellis, R. W. Johnson, E. S. Lewis). Finney: "I should say, too, that it is a more fatal disease with them, running a more rapid and virulent course, but this is simply my impression." Ellis: "Most of my cases in the negro have been secondary to tubo-ovarian inflammation in the female or to perityphlitic tuberculosis in the male."

The remaining twenty-nine afford more definite data for analysis. Our first division will include those who have never operated upon a negro for appendicitis, and there are four surgeons who have so replied (Bovée, Rees, Stokes, and Talley). Three of these reside in so-called black districts and one does not. As a second division we will take those who

have seen less than 10 cases, and we find eighteen reported under this head. A closer consideration shows that but one of these eighteen correspondents has had more than 6 cases (Leigh, 8 cases), while two have seen only 1 case (Rosser, Ruffin); three, 2 cases (Parham, Dean, Gessner); two, 3 cases (Grant, Martin [N. O.], Cathcart); two, 4 cases (Cartledge, Sherrill); four, 5 cases (Mastin, Simons, Baker, McGannon); three, 6 cases (Matas, Hill, Horsley).

As a third and last group may be selected those who have observed 10 cases or more, and seven replies were received to this effect. Further analysis indicates that one surgeon of the seven has seen just 10 cases (Guerry); one, 12 cases (Stone, who thinks the malady infrequent in negroes); one, 25 cases (J. W. Long, who considers it much more common in negroes than is generally believed); one, "numerous cases" (H. A. Kelly, reported by Burnam); and, finally, that three have operated upon negroes for appendicitis to the number of about 50 each (S. McGuire, "probably 50;" L. C. Morris, "between 50 and 60;" Balloch, 49). The first-named surgeon states that the disease is "much less frequent in colored than in white people," but the last two agree "that it is almost if not just as frequent as in whites" (Morris).

By far the most interesting and valuable information upon this whole question is that which has been courteously given me by Dr. Edward A. Balloch, of Washington, D. C., referred to above. With his kind permission the statistics of the Freedmen's Hospital for the past five years, with reference to this question, are here published for the first time, tabulated as follows:

| Year. | Abdominal operations. | Appendicitis. Acute. | Chronic. |
|---|---|---|---|
| 1900 | 46 | 6 | 5 |
| 1901 | 40 | 3 | 2 |
| 1902 | 48 | 3 | 3 |
| 1903 | 64 | 7 | 2 |
| 1904 | 60 | 3 | 4 |
| 1905 | 120 | 8 | 3 |
| | 378 | 30 | 19 |

Total, 49, or 13 per cent.

In addition to this, Dr. Balloch says in a personal letter: "In my last series of 100 operations for appendicitis, 14 were in colored patients. . . . I fully believe that if as much care were given to the examination of these patients as we give to our other cases, we should find very nearly the same proportion of cases in the negro as in the white."

Assembling these facts and figures, which I believe to be the most extensive and reliable yet produced, and looking at them in retrospect, let us see if any proper conclusions can be drawn from them. It may be seen at a glance that the majority vote is unquestionably on the side of those who believe in the rarity of appendicitis in the negro race. To support this view there were, out of twenty-nine observers, as many as twenty-two who had seen few, if any, cases, while two of the remaining seven had seen but a dozen or less. The experience of the other five operators, and particularly the last quoted, however, should receive considerable attention, for they have had abundant opportunities for observation and they have been especially interested in the subject. My own knowledge would lead me to believe with them that disease of the appendix is nearly as frequent in the negro as in the white population of the South. Of one thing I am quite sure, viz., that appendicitis is increasing in frequency among negroes—not alone relatively, but absolutely. Besides what I know of the work of others, it has been recited from my records that 3 cases occurred in the first seven and a half years and that 11 cases were operated on during the last two and a half years. The average negro does not seek medical advice early, and many cases of appendicitis either recover temporarily from attacks or die without coming to the surgeon. Numbers of them are, therefore, never seen at all. The education and progress of the colored race may account for the larger proportion of diseased appendices now being recognized—a comparative accession in numbers. But the same reasoning would apply to the white, in whom,

as in the negro, there is, to my mind, an actual increase of appendicitis cases. Significant comments by Senn,[1] in his letters from Africa, have served to confirm my views about this matter.

In speaking of a hospital in Mozambique, in which 2000 patients are treated annually, Professor Senn says: "Dr. Amaral has been stationed here for a long time and has never seen a case either of carcinoma or appendicitis in the black. The same holds true of the Eskimos of the far North, as I ascertained last summer when I went to Etah, the most Northern habitation of man. It seems, then, from the geographical limits of these diseases, that civilization with its complicated dietetics is responsible, to a large extent, for their prevalence in Europe and the United States." Senn also quotes Dr. Lacerda of another province in South Africa, as saying that he "has never seen a case of carcinoma or appendicitis in the negro, and believes that these diseases do not affect the race."

Thus, the question has a sociological as well as a medical side. The negro in this country is advancing with steady strides and being civilized simply by contact with the Caucasian race; isolated from the white man, he would be as he is now in Africa—free from the "race problem" and appendicitis. Whatever produces a diseased appendix in the white will produce it in the negro. All digestive disorders multiply as civilization goes forward, and the colored man with an inflamed appendix is no exception to the rule.

## DISCUSSION.

Dr. W. P. Carr, of Washington, D. C.—I take this occasion to apologize to Dr. Royster for not answering his letter, and the reason I did not answer it is because I intended to look up the statistics of the two hospitals with which I am connected, with a view to coming to some conclusions on this subject.

[1] Journal of the American Medical Association, September 1, 1906.

I have operated about twenty-five times on colored patients in Washington for appendicitis, and probably in just about the same ratio to the number of patients have my operations been on white people. I do not see that there is any difference in frequency of the disease in the negroes and in white people. I have operated on acute cases only in the negro. I do not think they come to the surgeon with chronic cases very often.

Dr. E. Denegre Martin, of New Orleans.—This subject is very interesting to me. I was surprised in looking up the statistics of the Charity Hospital, New Orleans, a few days ago, to find that appendicitis was so uncommon in negroes. For the past twelve years I have treated not less than 2000 colored women in that institution, and have operated four times for appendicitis. Two of the patients came in with appendiceal abscesses. I operated on one two weeks ago. This was an acute case. The appendix was seven inches in length, and the largest I have ever seen.

In looking over the statistics of the hospital, I conclude that appendicitis is not so prevalent in the colored race as it is in the white. I find in the records covering a period of eleven years, not including last year, that there were only 194 cases of appendicitis reported in the white race. In the last few years, however, more of these cases are being reported, and I cannot help believing that it is because we have been looking upon this subject rather seriously in the past five years, and looking for this disease. In that time only 77 cases have been reported of this disease in the colored race. Of that number, only 4 were females.

Just before leaving New Orleans to attend this meeting, I asked the house authorities their opinion with regard to the frequency of appendicitis in negroes, and they told me that their observations were the same as my own. I would know if there were any more colored females who had the disease, because I have charge of that entire service, and, as I have said, in that time I have had only 4 cases.

Another curious fact that impressed me was this: We have been looking for material for our classes, and I find that out of 20 odd cases of appendicitis recently reported, only 10 were operated on, 2 by the professors of surgery, and the remainder by members of the house staff. That is a condition of affairs which I shall endeavor to correct when I get home. (Laughter.)

I believe, possibly, the increase in appendicitis in the negro is due to the fact that they are today living pretty much as the whites do, and I think we will find, if we study the subject care-

fully and closely, most of the cases of appendicitis come from the cities and not from the country. The cases I had came from the city. It may be a coincidence, but I think that is worth noting. Civilization will add appendicitis to this race as it is going to add a great many other diseases.

Dr. R. E. Fort, of Nashville, Tenn.—I believe that appendicitis is just as frequent in the negro as it is in the white race. The reasons for this dearth of reports are twofold: In the first place, those of us who live in the South and know the negroes know that they only come to the hospital, not as the foreign pauper does here and in the East, where they are very generous to them, when they are practically dead. Usually, a slight attack of appendicitis means that the negro has "a misery" in his side. That is the real truth. That applies to the negro now with the same force as when, years ago, we were developing this subject of appendicitis, the surgeon said that the disease was twice or three times as frequent in the male as it was in the female. At least, surgeons then thought it was, but really did not know. Now, with more careful examinations and closer study and greater diagnostic acumen, together with increased experience, we know that appendicitis is just as frequent in women as it is in men. The reason for our mistake was a lack of training in this line, and the fact that we disregarded slight or catarrhal attacks of the disease. From a large experience in the Charity Hospital in Nashville, where 33⅓ per cent. of the patients admitted are negroes, I have frequently operated for appendicitis, and my experience leads me to make the statements which I have made, that all cases I have operated on have been for severe fulminating attacks of the disease. I have not seen slight attacks such as we see in patients of the better class of society. Time and again, I have opened the abdomen for pelvic lesions, and have found the appendix diseased. We find a similar state of affairs in the white race, too, but the patients have given no history of appendicitis. That corroborates what I have said, that we do not see these patients in the slight attacks, but they are just as frequent in the white as in the negro race.

Dr. R. M. Cunningham, of Ensley, Ala.—I really think the value of this or any other subject depends largely upon one's work, his environment, and his point of view. I will, therefore, give you these relative to myself before I make any criticism concerning this paper.

In the first place, my work has been largely among negroes.

For nearly twenty years I had under my charge from 700 to 900 negro convicts. As a routine procedure, postmortem examinations were made in all cases, and I cannot recall a single death from appendicitis. Operations were performed only a few times for appendicitis; and after the meeting at New Orleans in 1892, we began to make special investigations in regard to appendicitis, as represented by adhesions, etc., as at that meeting it was stated, I believe, by some gentleman that one-third of the race had appendicitis. For a number of years past, I have had charge of the medical service of an institution in which there are employed approximately 1000 to 1500 negro men. I presume with their families it would make 3000 or 4000 negro people coming under our observation. During this time there has been one death from appendicitis. In this case an enormous abscess had developed. The patient declined operation, and died with his abdomen full of pus.

As to the benefit to be derived from operation in these cases, I know I am wrong, or at least I shall meet with opposition, when I say that a firm stand should not be taken that every case of appendicitis is a surgical one. I do not believe that. I believe that many patients with appendicitis will recover, and that the vast majority do recover without operative intervention; and that there are many cases of appendicitis which neither the physician nor the patients themselves knew existed, and they got well. I recognize the fact that I am unable to say which case is medical and which is surgical. My experience has taught me that some patients die that would otherwise be saved. That I acknowledge.

As to the negro, if the disease is as frequent in the negro race as it is in the white race, and if it is a surgical disease, and without it there would be a large mortality, then the negro is an exception, for he has certainly not died with appendicitis under my observation. I believe there is a reason for this. The negro believes in purgatives, and if there is a free clinic where he can get purgatives he will attend it, for he is constantly looking for something to "work him" out. Those of you who have had experience with this class of people know that is true. They come to us and ask for such and such things. I have thought, without being able to establish it by statistics, and concluded rather dogmatically, that the frequent purging, as practised by negroes, acts as a prophylactic of appendicitis to a very great extent.

I do not believe that appendicitis is as prevalent in the negro

as it is in the white race. Certainly, I have had opportunities either at the operating-table before death, or in the dead-house after death, to learn this if practical observations of from 1000 to 4000 negroes for twenty years are worth anything. I find appendicitis rare in the negro race.

Dr. Stuart McGuire, of Richmond, Va.—I do not think statistics on this subject are worth much, especially when reported by a body like this. The men who constitute this Association do not cater to negro practice, and it is only the men, the members of this Association, who conduct clinics who have experience along this line. I believe appendicitis in the negro is rare, and although I stated in my report to Dr. Royster that I have operated on fifty cases, that number is small compared to operations upon white people during the same period. I believe the rarity of appendicitis in the negro is due to their good digestion. Those from the North who come South are not more forcibly struck with anything in the negro than his good white teeth. The first thing they say is, "How white their teeth are." The negro has good strong teeth, in spite of the fact that he never cleans them. And so he has a strong, stout appendix. The negro is not highly developed. He has not reached that period in evolution when his appendix is susceptible to disease. If Dr. Crile is present, I would like to ask him whether in his operative work on dogs he has found the appendix to be diseased? I believe that the stage of the negro's evolution with the appendix has not been evolved, but is in a process of being thrown off, and I think we are happy in being born at this age of surgical activity in that direction.

Dr. George Ben Johnston, of Richmond, Va.—I confess my derilection in not replying to the communication of Dr. Royster in regard to the frequency of appendicitis in the negro. My experience has not been large in operative work on the negro for the removal of the appendix. I am convinced, however, that the negro is not as frequent a victim of appendicitis as the white man is.

It will be recalled by a great many members of this Association that a few years ago the statement was made in all the works on genito-urinary surgery, stone in the bladder was an exceedingly uncommon malady in the negro, so uncommon as to constitute a surgical curiosity. Some ten years ago I made an investigation of this subject by communicating with the active operators throughout the South and collected a very large number of cases

of stone in the bladder to prove that stone in the bladder was almost as common in the negro as in the white. This error arose from the fact that the gentlemen who made our statistics lived remote from the negroes, and had had no experience with them; consequently, having had no experience with the negro, it was easy to see why they did not discover stone in the negro bladder.

That the negro does not have appendicitis quite as often as the white man, which is my belief, may be shown by circumstances, that he lives closer to nature than does the white man. The rural negro is less frequently the victim of stone in the bladder than the urban negro, and largely for the reason which Dr. Martin has alleged, that the city negro eats what his master does; whereas the negro living in the country, doing farm work, lives a wholesome life, and subsists largely on coarse vegetables and coarse meat. He is not the victim of digestive disturbances which may lead to appendicitis. Again, we might be disposed to regard appendicitis as a rare malady among negroes, largely owing to the fact that many cases in negroes are overlooked. When we consider that negro practice in the South is done chiefly by the inexperienced young, beginning practitioner, because he must get into the parlor through the kitchen, it is perfectly easy to understand why a great many cases of appendicitis should be overlooked. It is lack of diagnostic skill on the part of those who attend the negro which accounts for the small number of cases of appendicitis found in that race. In our Southern cities we are beginning to have a large number of negro doctors, and these are not of the most competent type. They attend the negroes. So those two circumstances, it seems to me, would answer for a great many failures in diagnosis. But even allowing for these failures, and in this way excluding a great many cases of appendicitis which absolutely occur, I am of the belief that the disease is not as common in the negro as it is in the white.

Dr. James E. Moore, of Minneapolis, Minn.—It has been brought out very clearly in this discussion that the negro suffers less frequently proportionally from appendicitis than the white man, and this is largely due to his environment and habits. But allow me to suggest that up our way most people operated on for appendicitis are known to have the disease because we are looking for it. We do not wait until they are nearly dead. When we find a patient with digestive disturbances we are always looking for an appendix at the bottom of the trouble, and commonly we find it. I may say, many cases of appendicitis in the

negroes are overlooked because they are not closely watched, and because we are not looking for that disease.

Dr. Royster (closing).—Just a few words in answer to one or two points that have been brought out, and which, it seems to me, have not been thoroughly cleared up. Dr. Martin's reply to Dr. Byford's remarks was correct, and I agree with him.

In regard to Dr. Cunningham's remarks, all I have to say is that his negroes are not civilized. (Laughter.) That settles the whole question. Dr. Senn is my authority for this statement. I have not seen appendicitis occur in constipated individuals more than in those who are not so. I think castor oil and other purgatives as a prophylactic against appendicitis even in white persons would not be an absolute guarantee.

Dr. McGuire speaks of the good teeth of negroes. I thought this myself until I consulted a great many dentists, and found negroes have bad teeth. Their front teeth are generally fairly good; these get a good deal of exercise from the things they eat; they use their hands and seldom the knife and fork. I would like to remark also that the negro's teeth seem whiter in comparison to his black skin.

One of the members of this Association has pointed out that the negro is more prone to diseases of the lower jaw than is the white race, and this is largely due to their bad teeth. Dr. Joseph Bloodgood, of the Johns Hopkins Hospital, is my authority for that statement.

Dr. Moore thinks we are not looking for appendicitis in the South. I have been looking for it for ten years; but the truth of the matter is that the negro is not looking for the surgeon. (Laughter.)

# SOME SUGGESTIONS IN REGARD TO THE SURGICAL TREATMENT OF EMPYEMA.

By Samuel Lloyd, M.D.,
*New York.*

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Four different methods of operating for empyema are usually described: First, aspiration or paracentesis; second, thoracotomy, with or without resection of a rib; third, the Estlander or Schede operations; and, fourth, the Fowler-Delorme method of decortication of the lung. To these must be added one recently suggested by Dr. Joseph Ransohoff, of Cincinnati, in the *Annals of Surgery*, April, 1906, which is simply a modification of the last-named method. This he calls "discission of the pulmonary pleura." It consists of "gridironing" the pulmonary pleura with many parallel incisions removed from each other about a quarter of an inch, and crossing these obliquely, or at right angles, with other parallel cuts.

Of the first two methods, little need be said at this time. They are of use in recent cases where the lung is not permanently collapsed and bound down, and will, if the lung expands, as it frequently does in these cases of early operation, result in a prompt and satisfactory cure. The other three methods are intended for the chronic cases of empyema where the lung has collapsed, and where its expansion to fill the pleural cavity is considered to be impossible. In these cases, according to the prevailing ideas, an attempt is made to compel the chest wall to fall in against the pulmonary pleura and become adherent to it in order to close the suppurating cavity; or, as in the last two operations, the lung itself,

released from its pleural covering, is made to expand so as to fill the suppurating space.

All these methods are based upon the idea that when the lung has once completely collapsed and become firmly fixed the pulmonary pleura loses its expansibility, owing to the inflammatory thickening, and cannot be made to sufficiently dilate to permit of the pulmonary and parietal pleural surfaces coming in contact. It is now over fifteen years since I began to experiment on these cases in order to determine if there was not some way to bring about this complete expansion of the lung without the extensive and mutilating operations upon the chest wall. In 1893, when Fowler and Delorme suggested their method, I discontinued experimental work for a short time and adopted the method of decortication, with the idea that this furnished the best solution of the problem. It was not long, however, before I was convinced, the difficulties attending the freeing of the lung from its pleural covering being so great and the result following the attempt so unsatisfactory, that it would be profitable to again take up the experimental work.

Tillman has said that "if in cases of old empyemata the lung, in consequence of prolonged compression, is no longer capable of expansion, the abscess cavity in the thorax cannot completely heal, and an empyemic fistula persists, as the rigid bony wall of the thorax does not yield to the cicatricial contraction, in the most severe cases the involved lung is found firmly contracted, the size of a fist, in the upper part of the pleural cavity, in these cases healing can only be secured by a sufficient resection of the ribs to enable the thoracic wall to yield and thus follow the cicatricial contraction." So also Dennis, in his *System of Surgery*, makes the statement that "in cases of empyema of long duration . . . the lung is fixed and cannot expand, the pleura is adherent and inelastic, and the chest is prevented from collapsing; and thus a cavity is permanently formed, the obliteration of which is necessary to effect a cure."

On the other hand, Forchheimer, in his recent work on the "Prophylaxis and Treatment of Internal Diseases," says that "as a matter of fact we do not find the lung collapsed in opening the chest for empyema, simply because the lung is always held by pleural adhesions;" but he does not say that frequently in these chronic cases we find the lung pushed up and held in the apex, as Tillman says, or else pushed inward and backward, and reduced to a ribbon-like mass, running parallel to the spinal column and attached firmly from apex to diaphragm in such a way that expansion is practically impossible. Forchheimer also makes the statement that Schede's operation would be uncalled for if the adhesions could be removed in some other way.

All sorts of devices have been invented in order to increase the intrapleural pressure and so cause the lung to expand. These undoubtedly have their place in those cases where the adhesions are few in number and recent in formation, so that they may yield readily to an increased pressure from without and the natural pressure from within the lung itself during respiration. One of my medical colleagues insists that any radical operation looking to a release of the lung from its adhesions is absolutely wrong; because he claims that as they contract they pull the lung nearer to the rib surfaces and so aid expansion.

Surgeons, however, who have had the opportunity of examining these cases as they occur on the operating table, will recognize the fact that the lung can no more get away from these adherent bands than Gulliver could rise from the cords of the Lilliputians.

Von Bergmann emphasizes the statement that we have all been taught by the physiologists that, if the opening in the pleural cavity is smaller than the diameter of the main bronchus, the lung will again take part in normal respiration. His explanation of this is that the air will enter the lung during inspiration through the main bronchus with greater ease than through the opening in the chest wall, so that

the collapsed lung is obliged to distend during inspiration. But if we agree with this statement it is impossible for us to do any work in the pleural cavity which involves a larger opening than the diameter of the main bronchus, without having a collapse of the lung.

Dennis also says that the pleural surface is insensitive, since it is changed into the wall of an abscess cavity.

It is evident, therefore, if we are going to make a success of any method of operating that calls for a re-expansion of the collapsed lung, it is necessary for us to invent either some method of operating in a vacuum, such as Sauerbruch's box, or prove that there is some method of bringing about expansion in spite of large openings in the chest wall and in spite of the other statements that we have quoted.

These were the difficulties that confronted me in the beginning of my work. One of the earliest facts, however, that impressed itself upon me, was that Dennis' statement in regard to the insensitiveness of the pleura was incorrect. It is perfectly true that under complete anesthesia we do not get a response to an irritation of the pleura; but this is not true, even when the pleura is markedly thickened and covered with plastic lymph, if the anesthesia is incomplete.

The next question that we had to determine was that of the elasticity of the pleura. If the generally accepted statements were correct that the pleura lost its elasticity in consequence of the thickening and the inflammatory deposits upon its surface, one would be obliged to adopt the methods of Fowler, Delorme, or Ransohoff, and in those cases where success did not result from these methods, recourse would have to be had to the Estlander or Schede methods.

We were able to demonstrate very early in our experimental work that these facts were also incorrect.

Having demonstrated that these preceding statements were true—that is, that the pleura in these cases was not insensitive and that it was still capable of expansion—the difficulty which presented itself was the physiological statement that with an opening larger than the diameter of the

main bronchus the atmospheric pressure from without alone would be sufficient to keep the lung in a state of collapse, and in that way our efforts to fill the abscess cavity by a proper expansion of the lung itself would be impossible.

We soon recognized that this was true in all of our fully narcotized patients, and it was by working on these facts on the living subject that we were able to perfect the method of operating that we have now employed for a number of years.

The opening in the chest is made in the usual way and should take in from one to three or four ribs, according to the size of the cavity and the difficulty in reaching the collapsed lung. In young children one rib is usually sufficient; in adults, from three to four are necessary. The piece removed should be from 2½ to 3 inches in length, and as a general rule the sixth, seventh, and eighth are selected. The pleura is incised, and the accumulated fluid allowed to drain away gradually at first. In all of these cases ether is the anesthetic of choice in my hands, for the reason that we can have the patient under complete anesthesia until the ribs are removed, and the effects of the ether narcosis last longer than the other anesthetics after stopping the administration. Before opening the pleura the anesthetic should be completely stopped, in order that if we get a sudden expansion of the lung by its breaking away from its retaining adhesions we may not get an overdose, and in order that during the remainder of the operation the patient may be gradually coming out from the effects of the anesthetic. As soon as the fluid has drained away, the opening in the pleura is made sufficiently large to enable the operator to make a thorough exploration of the whole pleural cavity and to accurately locate the position of the collapsed lung. If there are large masses of coagulated fibrin filling the cavity or adhering to the pleura, this should be at once scraped away, using a curette if necessary. The finger is then swept upward, if the lung is in the apex, until its margin is recognized and a separation of the adhesions is carried on in exactly the same way that we separate the

adhesions in the peritoneum. If these adhesions are so firm that they will not yield readily to the sweep of the finger along the pleural surfaces, the lung should be raised and a curved periosteotome swept along the parietal surfaces until the sensitiveness of the pleura asserts itself, and the now but partially anesthetized patient begins to cough with each sweep of the finger over its surface. With each forced expiration, expansion in the lung is seen to take place, until, when the adhesions are fully broken up, the lung with its pleural covering will fully fill the pleural cavity and even press outward through the operative wound. This is undoubtedly due to the healthy lung forcing a larger amount of air into the main bronchus than can escape through the partially closed glottis in the effort at coughing, so that this excess passes over into the bronchus of the collapsed side. In this way the healthy lung is used as an air-pump to expand the collapsed one. Some years after I had demonstrated this fact, Dr. A. H. Smith, of this city, told me that he had experimentally proven the same thing. He demonstrated it by taking two pairs of bellows to which he attached rubber tubes which were carried upward into a "Y," and this was in turn attached to a larger tube on which he placed a spring. If one pair of bellows was then emptied, air could be pumped with the other and escape without having any effect on the empty pair so long as the lumen of the larger tube was of sufficient size to allow of the egress of the full amount of air. As soon, however, as the spring on the larger tube was allowed to compress it, so that the full bellows-full of air could not escape at each compression, the air immediately flowed back and expanded the collapsed pair of bellows.

If the lung, instead of being in the apex, is compressed against the side and attached to the diaphragm, I have found it advisable to loosen the diaphragmatic adhesions first. These are usually much heavier and more difficult to separate than those in the upper part of the cavity. Care must also be taken here to recognize the margins of the lung and the curve of the diaphragm, both of which are some-

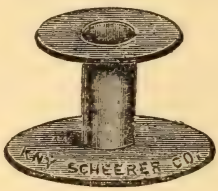
times very difficult to do. As soon as the separation from the diaphragm is complete, the other adhesions can easily be broken up by the finger, care being taken, however, when the empyema is on the left side, when one reaches that part of the operation where the pleura lies over the pericardium. Here the operator feels each contraction of the heart and can tell at once exactly how much pressure he can use in separating this portion of the lung. As a matter of fact, I have never had any difficulty during this part of the operation. It does not seem to cause any disturbance of the heart and does not offer any greater difficulties in the separation of the adhesions than any other part of the cavity.

As far as possible the operator should try to keep in contact with the parietal pleura during the separation of the adhesions, to obviate the possibility of tearing the lung. Sometimes it is impossible to avoid tearing off the pleura over a circumscribed area, and I have not infrequently produced a pneumothorax during the operation. This, however, no longer causes me any anxiety, as the healing process soon closes the opening in the lung, and, as a matter of fact, I have been successful in closing several cases of pyopneumothorax and getting a complete cure by this method of operating.

One of the difficulties that caused me a great deal of worry in the beginning was the fear of tearing into the lung and having a severe hemorrhage. I have, however, never had any difficulty of this kind in any of my cases where it was not possible to control the hemorrhage very readily. In case of opening a vessel in some portion of the lung that is not directly under the field of observation, the operator should at once have the anesthetist put back his anesthetic and bring the patient under complete narcosis. The lung will then again collapse, and the bleeding will either stop from this alone, or it can be found and controlled either by means of the Paquelin cautery or any other measure that may seem advisable. We have found that it is an aid in these cases to wash out the pleural cavity, sometimes two or three times during the course of the separation of the adhesions, in order to get

rid of the blood clot that results from the oozing from the denuded surfaces, and to get rid also of the coagulated fibrin that is set free during the course of the manipulation. This is always done by having hot saline poured into the wound from a pitcher, never from a douche nozzle or an irrigating bottle. In this way there is no increase in hydrostatic pressure, and the fluid poured in is washed around by the expanding lung and flows out again through the free opening at each expansion. As soon as the lung is fully expanded a drainage tube is inserted, the skin wound closed down to the tube, and a voluminous dressing applied.

We found in the course of our work that it was impossible in these cases to use a drainage tube of the old type. It almost invariably, by its rubbing against the pleure of the




expanded lung, kept up a constant pleuritic cough, another illustration of the fact that the pleura is not insensitive in these cases. Dr. H. D. Furniss, formerly house surgeon of the New York Post-Graduate Hospital, devised a tube while on the staff, to obviate this difficulty. This is simper a piece of rubber tubing with a flat piece of rubber on either end, made in different lengths to fit different thicknesses of the chest wall. In appearance it is very similar to a spool. The smaller flange of this tube rests against the inner wall of the chest, while the outer and larger flange rests upon the skin and prevents the drainage tube from falling into the pleural cavity. It is not necessary, before this audience, to say anything about the after-treatment of these cases. They are handled in the same way that is usual with other cases of empyema. A spirometer is useful in keeping up the full expansion of the

lung, and with children the small rubber toys made to blow up help materially with the subsequent pulmonary gymnastics.

I have now operated on 225 cases by this method. Of these, 97 were cured and 58 were improved. Of the 58 improved cases 40 were referred to the dispensary for dressing, and the great majority of them were ultimately completely cured; 9 were sent to the summer home; 9 were transferred to the Health Department Hospitals. One of these died some time after being taken home; the particulars are not known. 7 cases have been operated upon a second time, and it was surprising in these to find how thoroughly the lung had remained in full expansion. In one, an adult, operated upon for a very extensive empyema some years ago, a second operation was necessary because he lost his drainage tubes into his pleural cavity. In order to reach these tubes it was necessary to separate the adhesions on each side of the sinus where they had been lying, the pulmonary and the parietal pleuræ being in contact everywhere except at the point where the tubes had rested.

In many of my cases the history is incomplete, so that they have not been included in these statistics, only those where the history was absolutely complete being used.

There have been 47 deaths, or 20 per cent. This, of course, includes the deaths from all causes and for all periods of time after operation. The causes of death were:

| | |
|---|---|
| Nephritis, suppression of urine, and turpentine poisoning | 1 |
| Nephritis alone | 1 |
| Tuberculosis | 3 |
| Pyemia, nineteen days | 2 |
| Enteritis and pneumonia | 1 |
| Attack of vomiting three weeks after operation | 1 |
| Pneumonia, both sides | 2 |
| Collapse on opening chest | 1 |
| Diphtheria | 3 |
| Volvulus | 1 |
| Empyema and abscess of lung | 1 |
| Shock | 8 |
| Exhaustion, from two to forty-two days | 22 |
| | 47 |

If we take into account the cases dying within one week of the operation as due to the effects of the operation itself, we get 15 cases. The others in the above list, in all probability, would have died no matter what method of operation was undertaken. The length of time the patients have remained in the hospital has varied from six to forty-two days. The average has been twenty-nine days.

In children under eighteen months of age the mortality is very much higher than in older cases.

The shock following this operation is distinctly less than in the other operations for chronic empyema. Within two or three days the patients are able to sit up, and they are usually out of bed within a week.

## DISCUSSION.

Dr. Edward H. Ochsner, of Chicago.—The position taken by the essayist is so absolutely and diametrically opposed to the one which we have followed for many years, that I cannot refrain from saying a few words on the subject. First of all, I believe that almost all cases of empyema that come to the table for a second operation do so because of the poor work done at the first operation or because of some fault in the after-treatment. It does not make any difference of how long standing the empyema is, providing only that there are no extensive calcareous deposits in the walls of the cavity. I have no exact records on hand, but I do not believe that we have as many cases of empyema as the author has reported, although we have had a good many in the last twelve years, and in that time we have had to do only one secondary operation on those cases on whom we had done the primary operation. This was a case with extensive fibrocalcareous deposit reaching an inch in thickness in some places. I repeat: When a patient comes for operation a second time it is almost always because of imperfect work done the first time. We have had to operate on many cases that had been operated on two or three times previously and I am convinced that in most of these cases either too much or the wrong thing was done the first time. Some of the many harmful things that can be done in the treatment of empyema are irrigation, insufficient

drainage, continuous aspiration, and among them I would include the extensive operation advocated by the essayist. I would condemn them because they are unnecessary and that word to me is synonymous with harmful. If through-and-through drainage is instituted, and the drain is left in long enough, a permanent cure will almost invariably result, providing, of course, not too much has been done at the primary operation. These extensive operations, especially if coupled with irrigation and caustic solutions, make such strong adhesions that they prevent proper expansion of the lung later; while if these strong adhesions are prevented, the action of the diaphragm in its effort to produce a vacuum is almost sure to secure the proper expansion of the lung ultimately. Furthermore, I should be afraid that this extensive operation advocated would increase the number of cases in which a communication would be established between the empyema cavity and a large bronchus, one of the worst calamities that can befall one of these patients.

Dr. W. P. Carr, of Washington, D. C.—Dr. Ochsner's experience corresponds with my own. I have operated on 23 cases of empyema in the last two years, and in every case the lung has expanded, and the operation has been nothing more than simple drainage of the chest cavity thoroughly carried out. Even if the lung does not expand completely, the cavity will fill by pushing over the pleura, pushing up the diaphragm, by resecting a rib to some extent. I had a good illustration of how a large cavity could be filled up in one case, in which I found, instead of empyema, gangrene of the lung. The entire lower lobe of the right lung was gangrenous, and I cleaned the cavity out with my hand as well as possible and instituted thorough drainage. The patient was exhibited at a meeting of our local medical society in Washington. The diaphragm has come up to the level of the nipple, and he is perfectly well in every respect. My plan is to put one long drainage tube to the back of the thoracic cavity, and another one of the larger diameter through the chest wall, and in a good many cases large pieces of lymph came out through this larger drainage tube, and continued to come out for sometimes a week or more. Many of these pieces of lymph found in the pleural cavity were as large as my fist, and they do not begin to come away until nearly a week has elapsed. But there is one thing that ought to be remembered, that these cases of large empyemas ought not to occur. In the treatment of pleurisy every case ought to be aspirated when the effusion gets large, above the lower angle of

the scapula. If that is done, we will never have extensive compression of the lung and the formation of adhesions in that way. If aspiration is done when the fluid gets copious, of course empyema would be detected at once, and then the chest ought to be drained immediately by the resection of one rib, which is just as good for this purpose as the resection of two or three ribs.

Dr. A. M. Cartledge, of Louisville.—I have enjoyed Dr. Lloyd's paper very much, and I regard it as an able contribution to this subject. I think it would have been fortunate had the essayist classified the cases of empyema better than he did. The impression that one gets from the paper is that he treats all cases of empyema he meets with in about the same way; and I must agree with Dr. Ochsner that in many of the cases—at least, those I have seen—such extensive surgery is unnecessary.

I was very much impressed by the statement made in connection with Dr. Lloyd's statistics, that the disease proves most fatal in the young, particularly young children. This class of cases we all see many times, and yet just the reverse has been my experience. In these cases of rapidly-forming empyema in children, following usually pleuritis and infection, I have obtained the most satisfactory results by the simpler form of operation, namely, the resection of an inch or so of a rib. We should pay the greatest attention to the classification of these cases, for there is just as much difference in a case of old empyema which comes to us with a caved-in chest cavity, that has had two or three operations, with a great many adhesions, as there is in an empyema which presents itself for the first time. In fact, I would put it a little more strongly, and say that it is a totally different proposition from a case of empyema which comes to us for the first time. In simple cases the removal of a section of a rib, an inch and a half, say, of the seventh rib, with the insertion of a large drainage tube and employing a protective covering, will answer the purpose in the majority of cases. I do not think we should ever resort to extensive multiple resection of ribs until such an operation as I have mentioned has been tried. In the vast majority of cases we will succeed with a more simple operation, and I think the more extensive operations in children perhaps account for the great mortality. They cannot bear the shock of an Estlander operation, where the opening is sufficiently large to pass the hand into the thoracic cavity to determine the presence or not of adhesions.

Let me say a word or two in regard to the old cases, with sinus

or sinuses, that have been operated on and still continue to discharge. These are in a class by themselves. Some of them are very rebellious to treatment. I think the essayist had these in his mind when he wrote the paper. One cannot adopt the plan of simply making an opening and inserting a drainage tube in these cases, for the reason that, first, he has to do an extensive resection of the ribs to get in there, break up bands of adhesions, and they are tenacious; in the absence of calcareous deposits, these furnish strong evidence for extensive resection. But even in the absence of calcareous deposits, the lung is so bound down that I do not believe drainage would afford the relief desired. I believe that is the consensus of opinion regarding extensive operation and observation on this subject.

I want to emphasize this point, that in the intractable cases that go from one surgeon to another, if we ever expect to cure them, we must bring the lung to the chest wall, or take the chest wall to the lung. That is the important lesson in these cases. It is necessary to do extensive surgical work in this class of cases, making an opening large enough to introduce the hand. It has been my experience that the text-book idea of a contracted lung in such cases is wrong. It is not a little ball like a fist, but a long strip running along the spine, and the adhesions are quite as extensive below and to the diaphragm as above. My experience is that the adhesions are more posterior than anterior in these cases, and that drainage alone will not relieve the adhesions so as to permit of expansion of the lung, consequently I have had to cut them with scissors. Finally, I feel that in order to effect a cure in cases of this kind, extensive rib resection must be done, which will absolutely loosen the bands of adhesions, followed by extensive drainage.

Dr. Lloyd (closing).—I have been a little surprised that the gentlemen who have discussed my paper have treated me so leniently. I expected an attack from Dr. Ochsner. When we get away from the idea of collapse of large openings in the chest wall, and the contracted and insensitiveness of the pleura, we will recognize that something can be done for these cases.

Dr. Carr spoke of making a small incision and instituting drainage. A small incision and the establishment of drainage are not sufficient to cope with the class of cases I have described.

Dr. Cartledge criticizes my classification. He must have misunderstood what I said, and this misunderstanding may have been due to the rapidity with which I read my paper, or,

perhaps, my language was not clear. I was surprised and gratified to hear him say that he had carried out the same course of treatment in the class of cases mentioned and had arrived at the same conclusion independently from his own experience. That the lung will go down in these cases with any drainage we choose to put in the pleural cavity is false. It cannot. It is an utter impossibility for it to get away from strips of adhesions thicker than two fingers, and they must be cut, as Dr. Cartledge has said, with a pair of scissors. The diaphragm goes up, the chest falls in, and fortunately in many of these cases we come in contact with slight oblong areas of the lung independent of pleuritic adhesions. The pleuræ and pulmonary pleuræ coming in contact we get obliteration of the suppurating surface. What happens in those cases when Dr. Ochsner carries three tubes through? He gets the diaphragm and the lung to meet, and this leaves two spaces in which he can have an accumulation of fluid which will keep up a sinus indefinitely, giving us the chronic empyemic fistula that Tillmann and others talk about. It is these cases that come to a secondary operation. A secondary operation, too, is required occasionally for those cases in which we get necrosis of the rib, and which do not frequently involve the inside of the pleural cavity. The idea is simply this: I think we are in accord, that a simple thoracotomy or paracentesis in the early cases will cure the vast majority; but in those cases where we have the lung collapsed and bound down, it is better to do a more extensive operation and cut through the pleura.

# FOREIGN BODIES IN THE URINARY BLADDER. CYSTOTOMY.

By C. E. Caldwell, M.D.,
*Cincinnati.*

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The literature of foreign bodies in the bladder is very voluminous, and I shall not trespass on your time and patience to attempt the description of the innumerable objects which have found lodgment there.

The annals of insane asylums are replete with the reports of cases in which the most inconceivable objects have been introduced through the urethra in both male and female patients. The female urethra being so much shorter and more patent, foreign bodies are not only introduced with greater ease but their detection and extraction is attended with less difficulty. The attempt to use a douche by a patient unfamiliar with her anatomy was followed by the loss of the douche nozzle in the bladder.

Foreign bodies may ulcerate into the bladder from contiguous parts, as, for instance, ligatures have been found in the bladder that were used in ligating the pedicles of ovarian cysts, fecal calculi from adherent and ulcerating bowel, and pessaries have ulcerated through from the vault of the vagina.

By far the most frequent source of foreign bodies in the bladder in the sane, or those not addicted to acts of sexual perversion, is the loss by breaking off or detachment of the end of an instrument introduced either by the patient himself or his physician. Broken bougies, catheters, or lithotrite beaks may, therefore, be found in the bladder. Unless there

be some reason for concealment, the anamnesis will usually discover the cause of the symptoms which induce the patient to seek relief of his physician.

When the history is clear of a foreign body in the bladder, a cystoscopic examination will readily corroborate it. In certain cases, however, a cystoscopic examination may not be desirable or practicable. When the foreign body has been long in the bladder, and sufficient time has elapsed for its incrustation with earthy salts, and when the patient has become enfeebled by the persistence of aggravated bladder inflammation, a much more ready method of detection and definition of the foreign body is by the skiagraph. If this should reveal a heavily incrusted foreign body the question of selection of the method of removing it will depend on several conditions. In the class of patients in which the cystoscope is contra-indicated it is quite obvious that nothing is to be gained by attempts to remove the body by lithotripsy, as the trauma resulting therefrom in an already infected and weakened patient might have serious consequences. In such cases cystotomy seems a more desirable alternative. The question of the route will depend largely on circumstances. The suprapubic route certainly commends itself as the easier of approach and attended with less serious immediate consequences to the patient.

In uncomplicated cases, where there is no obstruction, such as an enlarged prostate, to the neck of the bladder; when sufficient time has not elapsed for the incrustation of the foreign body with lime salts, and when the object is of such diameter that its extraction may be effected without too much damage to the urethra, the operating cystoscope offers everything in the way of facilitating the removal of the object through the urethra.

In the case which I here report, the patient was old, feeble, a prostatic with an infected bladder, a bad pulse, bad kidneys, and almost exhausted by the pain of frequent and interrupted micturition. Believing that the length of time that

Catheter ends in bladder.

had elapsed—four weeks—since the accident of breaking the elastic gum catheter in his bladder was sufficient to have caused an incrustation of the catheter ends that would interrupt the *x*-rays, I sent him to the *x*-ray room of the hospital, where our skiagraphist, Dr. Sidney Lange, succeeded in getting a very satisfactory skiagraph of the bladder and its contents. The position of the catheter ends was transverse to the long axis of the bladder, as is usually the case, and had it not been for the prostatic obstruction and the extreme feebleness of the patient I should have considered this an ideal case for extraction with the aid of the cystoscope.

The temptation to do a prostatectomy, once the bladder was opened, was great, but I was deterred by the consideration that a live patient is preferable to a dead one with his prostate removed; but I may have been too cautious, and am still wondering whether I would have added much to the chances against his recovery by removing the prostate. However, I hope to do this subsequently.

J. B., aged seventy-one years, was admitted to the hospital August 31, 1906. Patient has had trouble in retaining his urine for some time. Having trouble in completely emptying the bladder, whenever he would go out he would use a catheter. About a month ago on using the catheter it broke, leaving the end in his bladder. Since this time he has had great trouble in voiding his urine, being able to pass only a few drops at a time; then, on walking about, was able to void more.

*Examination.* Patient poorly developed and nourished; general condition and appearance bad; senile changes marked. On passing sound (which enters without difficulty) cannot completely turn about. Seems to come in contact with a thickened bladder wall; no clink found. Prostate found enlarged.

*Urine.* Cloudy, amber, 1012, alkaline, albumin present, no casts. Large quantity of pus cells found.

Catheter, p. r. n. Morph. gr. $\frac{1}{4}$. Tinct. belladonna, gtt. x.

*September* 1*st*. Doing well. Had severe vesical tenesmus and temperature rose to 103°. Morph. gr. $\frac{1}{4}$ given repeatedly, which quieted him. Toward morning catheterized.

2*d*. Temperature normal and quiet.

5*th*. Bladder irrigated twice with boric acid solution. *X*-ray shows two pieces of broken catheter.

7*th*. Under ether anesthesia, incision made over pubis transversely for about three and a half inches. Previous to this bladder had been filled with water. Incision carried down through cellular tissue to bladder wall, which was grasped and knicked. Incision in bladder enlarged, and with forceps both pieces of catheter removed, having incrustations of calcium phosphates. Bladder wall then sewed to cellular tissues and muscle with thin catgut at both angles (lateral). Sutures carried through skin; retaining catheter inserted and united to skin by silkworm-gut. Wound dressed and patient sent to ward, where he was put in semi-upright position. Catheter united with tube leading to a jar beside the bed, so as to facilitate drainage. Redressed in the evening; dressings saturated.

8*th*. Attempted to irrigate bladder; fluid, however, passed beneath cellular tissues of abdomen and irrigation abandoned. Several sutures about wound cut so as to facilitate drainage of extravasated fluid. Redressed in the evening; abdominal distention marked; nauseated, and coughing up slime. Food unretained. Strychnine continued. Pulse fair, temperature 100°, highest. Podophyllin, gr. j, giving no results, simple enema in the evening with fair result.

9*th*. Distention still present to less extent; still nauseated and gagging; resting more quietly; no movement of bowels; purgative enema given with good result; general condition fair.

10*th*. Has not been able to retain food. Half an ounce of whisky with strychnine three times hourly was retained. Distention still present. At 9 P.M. turpentine, gtt. v given, which caused great nausea and gagging. Tube came out and had to be replaced. Wound looks fairly clean.

11*th*. Considerably improved. Distention less. Bowels moved voluntarily. Wound looks clean. Catheter taken out and cleansed.

13*th*. Wound healing. Patient able to eat something. Bowels constipated; salts given.

15*th*. Improving locally and generally.

20*th*. Wound closing nicely. Catheter taken out and cleansed daily. Daily irrigation of bladder through penis with weak permang. potas. sol. Urine still alkaline.

25*th*. Improving. Treatment same.

*October* 1*st*. Catheter allowed to remain in penis. Wound much smaller above; small catheter used in suprapubic opening. Irrigated with normal saline solution and $AgNO_3$ $\frac{1}{1000}$ daily.

5*th*. Improved. Urine less ammoniacal. Treatment same.

10*th*. Catheter in suprapubic opening taken out; became quite wet, as urine drained to some extent in upper opening.

15*th*. Less wet from opening above.

20*th*. Has been dry for two days.

25*th*. Dry since the 18th. Catheter taken out of penis. Able to void nicely.

27*th*. Became wet again from suprapubic opening. Catheterized and bladder found full, having had incontinence of overflow. Catheter allowed to remain.

30*th*. Dry again since catheter was allowed to remain.

*November* 4*th*. Still dry. Catheter removed. Patient allowed to get up; voids voluntarily.

9*th*. Condition improved.

10*th*. Found improving with catheter inserted to make continuous drainage. No urine has passed through suprapubic opening for several days.

19*th*. Catheter had been out for three days and the man was voiding naturally, no urine had come by suprapubic opening for ten days, when suddenly the old opening broke

out again and urine flowed through it enough to saturate two towels. Replaced continuous drainage per catheter.

22*d*. Man dry as long as catheter is kept in urethra. Insists on going home to-morrow.

I was on duty only until October 1, when the patient passed into the hands of Dr. N. P. Dandridge, whose service I was filling.

The prostatic obstruction was the cause of urine retention on removal of catheter. The retention was the cause of the opening of the bladder fistula. If the patient will now consent to a removal of his prostate, I feel assured that he will have no further trouble with the urinary fistula.

My object in presenting this isolated case is to elicit the discussion as to the best method of procedure in these cases.

## DISCUSSION.

Dr. George H. Noble, of Atlanta.—I desire to call attention to the danger of leaving the head of self-retaining catheters in the bladder either through oversight or neglect; as rubber deteriorates rapidly in warm climates, the liability to this accident is greater in the South than in colder climates.

It is my intention to refer to the common practice of introducing self-retaining or button-head catheters, and leaving the removal to assistants or nurses, who, not knowing the end of the instrument, draws upon the stem of the catheter in the act of removal, and pulls it off from the expanded head. The latter, therefore, is unwittingly left in the bladder, and if neglected becomes a nucleus for a calculus.

I have seen several accidents of this sort, and have found and removed several pieces of the catheter that had not been suspected.

When such an instrument is employed, the assistant or nurse should be informed of the danger of this accident, and be instructed to see that the head of the instrument is removed intact and presented to the operator for inspection. In case it is known that the head of the catheter is left in the bladder, its removal through a cystoscope is a matter of easy execution.

Dr. Randolph Winslow, of Baltimore.—About a year ago I

removed three calculi from the bladder of a man who had passed a wire into his urethra. One calculus was nearly as large as a hen's egg. Another one was situated partly in the bladder and partly in the prostatic urethra, and a third, somewhat irregular in shape and size, was lodged in the prostatic sinus.

There is nothing special to be said in regard to this case, except that this man was young, and on account of some abnormal sensation (I do not know what it was), he concluded he would pass a piece of wire through his urethra and see what the effect would be. He passed some feet of fine wire into the urethra; it slipped, or in some manner he lost hold of it, and it disappeared. For four years he carried that wire in his bladder, suffering more or less during this time, and it was with considerable difficulty that he was persuaded to have an operation done for its removal. I opened the bladder above the pubes, and removed the calculi.

Dr. Stone.—I would like to ask Dr. Caldwell whether he does not think the suprapubic route is the most desirable in these cases?

Dr. Caldwell (closing).—I was anxious to bring up the question of route and whether I had acted properly in this case, as I might have added very much to the dangers of the operation by the simultaneous removal of the patient's prostate. I thought it was good judgment to let the prostate alone, owing to the patient's age and enfeeblement. Perhaps there are others who may think that I should have relieved the man of his prostate, and I am seriously in doubt whether it would not have been better done.

# HARMFUL INVOLUTION OF THE APPENDIX.

By Robert T. Morris, M.D.,
*New York.*

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My effort to catch this subject in the screw thread of professional interest two years ago did not succeed, because of the title of the published article, "Normal Involution of the Appendix." No one seems to be much interested in anything normal associated with the appendix.

Senn and Ribbert first called attention to the fact that the appendix vermiformis normally undergoes an involution process, with replacement of the lymphoid, mucous, and submucous coats by connective tissues. They attached no clinical significance to a condition which sends many thousands of patients to the doctor every year. A study of the subject brought out the fact that nerve filaments persist longer than other structures in a disappearing appendix. Contraction of the hyperplastic connective tissue irritates these nerve filaments in the appendix just as it does in any sort of scar tissue elsewhere in the body. The observation pressed a button that suddenly turned on the light in a dark part of the abdomen.

Irritation of persisting nerve filaments in the involuting appendix gives us a clearly diagnosticable class of cases that go the rounds of the profession with intestinal dyspepsia. Very often these patients go from physician to physician, without having the part of Hamlet observed in the play. In other cases the appendix takes the form of a question mark, and the patient goes from surgeon to surgeon. Like Mar-

guerite with the petals, the question runs: "He will operate; he will not operate; he will operate; he will not operate."

Whether the appendix is eventually removed or not will depend upon the personality of the last speaker in the case. Patients do not go to bed with their involuting appendices. The process is an irritative one, and is not an infective one. Further than that the involution process seems actually to guard patients against infective processes in the appendix, although these patients are quite as apt, as any others, to ask to have the appendix looked after. Many a surgeon has operated in these cases, and has removed what he feared was a normal appendix. It went into the waste basket instead of to the pathologist. The surgeon regretted that he had made a mistake in diagnosis, but the patient gained forty pounds in weight, and paid his bill. That does not satisfy a conscientious surgeon, but it sets him at thinking. The pathologist can give him an object lesson. He can show that the inner coats of the appendix are undergoing replacement by connective tissue; that nerve filaments persist abundantly, and that the nerve filaments are surrounded by such groups of new cells that the irritation feature stands out in colors upon the map.

Normal involution of the appendix may begin in the early years of life, but most of the patients whom we see with this condition are past twenty-five years of age.

Connective tissue replacement of the appendix may, and usually does, progress evenly from the distal extremity.

Fig. 1 shows an appendix from which all normal structures, excepting peritoneum, nerve filaments, and capillaries, had disappeared. The lumen was obliterated wholly in this case. The patient had suffered from "intestinal dyspepsia" for about ten years. She was immediately cured on removal of the appendix.

Fig. 2 shows an appendix of a rather rare type, in which involution was progressing in the form of nodes. The bulging parts in the cut represent internodes of mucosa

and lymphoid tissue, while the constricted parts represent nodes of connective tissue replacement of such structures. No lumen was left patent at the nodes. This patient had suffered for some years with intestinal dyspepsia, and with rather more than ordinary discomfort in the appendix region. He was cured at once by removal of the appendix.

In most of the cases we find the proximal portion of the appendix still patent and carrying a thin layer of mucosa, with involution changes most marked toward the distal extremity.

Fig. 3 shows a cross section of the appendix of Fig. 1. The circular line of cleavage between former tissue planes differentiates the appendix that is undergoing an involution process from the scarred appendix which is found after an infective appendicitis.

The symptoms of normal involution of the appendix begin gradually, and are usually given the dignity of the diagnostic entity of "intestinal dyspepsia." The appendix is sometimes included as a possible causative factor, but is usually left out.

What are the symptoms of normal involution of the appendix? They may be divided into two groups:

First, the group of the endless chain of symptoms running out of the hawse hole of autointoxication.

Next, four links of the chain which suffice to hold us short up to the diagnosis. These are the four links: (1) Intestinal dyspepsia; (2) a sensation of discomfort in the appendix region; (3) an appendix that feels narrow and hard on palpation; (4) hyperesthesia of the right lumbar plexus of nerves.

Now let us elaborate a bit.

1. What are the characteristics of intestinal dyspepsia of involution appendix origin? Persistent distention of the cecum and ascending colon with gas. Irritation of the entrapped nerve filaments of the appendix irritates the intimate ganglia of the bowel wall (Auerbach's and Meissner's

Fig. 1.—Symmetrical involution.

Fig. 2.—Nodular involution.

Fig. 3.—Transverse section of the appendix, showing replacement of the inner coats by connective tissue.

plexuses) and apparently causes the greatest degree of disturbance in that part of the bowel which is nearest to the original point of irritation.

2. The sensation of discomfort in the appendix region is characteristic. The patient has a tendency to press upon the abdomen at that point with his hand, or to lean against a table occasionally. The sensation may pass away for a few hours, or even for a few days at a time, but it is soon back again, and it lasts for years. Formerly I told patients that their discomfort was caused by the presence of gas in the cecum and ascending colon. I was wrong about that. Now when I hear other physicians tell patients the same thing I know that they are as wrong as I used to be.

3. The appendix that is undergoing involution changes feels harder on palpation than a normal one. So does an Edebohls' appendix ("Congestion caused by the pressure of a loose kidney upon the superior mesenteric vein"). So also does a scar appendix. All three appendices cause symptom No. 4 (hyperesthesia of right lumbar plexus).

How are we to separate the three types of appendix?

The scar appendix goes with a history of previous infective invasion. Edebohls' appendix feels plump on palpation—the plumpness of intestinal exudation. It is often tender on pressure, and a loose right kidney is found with it.

The appendix which I describe feels narrow and hard on palpation—the hardness of hyperplastic connective tissue. It has no history of infective invasion, and there are times when it is not at all tender on pressure. Further, the scar appendix may be the seat of acute or chronic infective invasion at any time if lymphoid tissue and mucosa remain. Edebohls' appendix may be the seat of acute or chronic infection at any time, although it usually gets up protection. The appendix which I describe is not at any time the seat of acute or chronic infection, so far as I am able to learn.

4. In the scar appendix, in Edebohls' appendix, and in the appendix which I describe, there is one constant diag-

nostic sign in hyperesthesia of the right lumbar nerve plexus. Press upon the abdomen at a point about one inch to the right and to the left of the navel, carrying the finger points well down toward each side of the spinal column. This will make pressure upon the right and left lumbar plexuses. The patient will tell the rest of the story.

From what other causes are we to differentiate the intestinal dyspepsia that goes with involution of the appendix?*

(*a*) From scar appendix and from Edebohls' appendix, by the features already described; (*b*) from bile-tract adhesions, "gall-spider cases," by the predominance of gastric dyspepsia in the latter, and by tenderness and muscle protection of the gall-bladder region; (*c*) from eyestrain cases, by the predominance of gastric dyspepsia, and by having the possible eyestrain factor worked out by an ophthalmologist who reports upon muscular imbalance rather than upon "the sight;" (*d*) from visceral ptoses, by the ptoses in evidence.

It is not necessary to go into further details, because bile-tract adhesions, eyestrain, visceral ptoses, and appendices undergoing involution cause such a large proportion of all dyspepsias that one who is in the habit of eliminating these causes in a diagnostic way will have his own methods for following up other dyspepsias.

What are we to say to patients who come for advice about appendices that are undergoing harmful involution? My own custom is to tell them that it is not necessary to have the appendix removed, so far as securing safety is concerned. It is merely a matter of expediency. Before I understood the subject I sometimes told patients that the appendices which were always growling might better be removed in order

* Blake described in the Annals of Surgery for September, 1905, similar symptoms, caused by faulty mesenteric attachment of the appendix, and some years ago I ascribed such symptoms to "torsion of the appendix." At that time the appendices had not been examined microscopically, and they may have been appendices undergoing involution changes.

to avert possible explosions in the future. This advice was based upon a wrong basis. Now I tell the patients that if their physicians can keep them comfortable and maintain a good general health standard, there is no need for operation—even though the operation is rather unimportant as surgical operations go. The patients leave the office relieved of their apprehensions, but a good many of them will turn up again in about a year and say that they are tired of making it a business to keep well. They have too many other things to do. They get along pretty well under medical treatment, but they weary of keeping it up. Friends get after them and persuade them to obtain a remnant lot of advice, good, bad, and indifferent.

In my first published contribution to the subject of the appendix that is undergoing normal involution changes, reasons were given for dissuading physicians from adopting the nomenclature which had been adopted by some of my assistants and by some of the physicians who, for convenience' sake and for brevity, had given this type of appendix my name. The nomenclature went wrong and got to stand for normal appendices. That will not do at all. I have never at any time, in speaking or in writing, advocated removal of the normal appendix, even when it appears in the field conveniently during the course of some other operation. My dictum has previously been, leave the appendix alone until it is infected, and then lose no time in having it inspected.

Of late I have had to make a change, and now remove uninfected appendices that are undergoing normal involution changes, provided that the patients and their physicians are agreed upon making the request. It is a different attitude from the one that the surgeon is bound to take, on moral grounds, when a case of infective appendicitis comes under his care and responsibility.

It is a pity that the nomenclature for the appendix that is undergoing involution changes went wrong in two instances;

when the title of normal involution of the appendix failed to engage professional interest, and when such an appendix called by my name got to stand for a normal appendix. Mr. Cleveland has said that in social and in political economics nothing is more dangerous than phrases. In medical economics nothing is more dangerous than nomenclature. The term "wood alcohol" has caused deaths and blindness because few people are scared by wood, and fewer still are afraid of alcohol. The term "gonorrheal rheumatism" has led to giving patients antirheumatic treatment when that sort of treatment was exactly what they could not bear. Increasing the natural physiological resistance of the patients would have been found to be the keynote to successful treatment of the cases if the word "septicemia" had appeared in place of "rheumatism." The appellation of "catarrhal appendicitis" has caused endless deaths and disaster, because it was the sand in which so many advisors hid their heads when gangrene and perforation of the appendix were impending. I have operated in very many cases of appendicitis that had been diagnosticated as "catarrhal," and have seen only one or two that could properly and safely have been classified under that title.

Let us get right on the nomenclature of normal involution of the appendix, and if it is called by any more brief term, let us not under any circumstances have it stand for the normal appendix. The subject is as important as any other subject with which the physician has to deal regularly in his daily routine of work, for there are few cases of any one sort which one will see more often than cases in which the appendix is undergoing harmful involution changes.

## DISCUSSION.

Dr. Samuel Lloyd, of New York.—There is one thing that should be said in this connection, and that is Dr. Morris has drawn a picture that is a little too hopeful, as I think there are

a certain number of cases of involution of the appendix that become bedridden. Very recently I had an interesting experience in that line. A few months ago I was called very urgently to see a patient who had been in bed for weeks with pain in the right iliac fossa, gradually increasing, and which continued on for a long time. I jollied her along and kept her from undergoing operation; but finally she had so much discomfort and the pain was so constant that it was impossible to delay operation any longer. Operation was accordingly done, and we found an almost completely involuted appendix. It was removed, and all discomfort disappeared. She was not a neurasthenic. She is a very active woman. I have seen a number of cases where constant nerve irritation through the appendix created a considerable amount of discomfort in the intestinal canal, until the patients have insisted upon operative relief.

Dr. Lewis C. Morris, of Birmingham, Ala.—There can be no question but that cicatricial scar of the appendix frequently exists, and is usually associated with digestive and nervous symptoms. I think the most important differentiation to be made is the differential diagnosis of the psychological appendix, the one Dr. Osler has written about. This differential diagnosis must be made, because I feel confident that all of us see cases of appendicitis that are purely psychological, where unquestionably the condition is due entirely to nervous influences and to suggestion by the occurrence of appendicitis in friends or people with whom they have come in contact. These cases do exist. Removal of the appendix will relieve the symptoms when we have this scar of the appendix.

Dr. George H. Noble, of Atlanta.—Granting that normal involution of the appendix does occur, the liability to err in differentiating between it and chronic colitis is very great.

In the first place, because colitis is a very common disease and is commonly overlooked by the surgeon, if, therefore, his attention is called to normal involution of the appendix, it should also be directed to colitis, especially as the pain about the cecum does not differ much in character or intensity. In addition to the usual symptoms of colitis, there is one I regard as of considerable value in diagnosis, that is, palpation by a rolling motion of the cecum under the tip of the fingers, which will stimulate it to contraction until it feels more or less like a soft, boggy rope, the outlines of which quickly disappear upon cessation of palpation.

When these signs occur in connection with other symptoms

of colitis, it is not safe to say that the disease is due to any form of affection of the appendix.

I have known a number of these cases to be operated upon for chronic appendicitis through an error in diagnosis. They get no relief from such operations and continue to suffer. Upon the other hand, if the disease should be mistaken for normal involution of the appendix, the patient may be advised against treatment and unwittingly deprived of necessary attention to existing colitis.

Dr. Edward J. Ill, of Newark, N. J.—Dr. Morris has drawn our attention especially to the question of nomenclature. I have called the form of appendicitis, or condition of the appendix, Dr. Morris has described, cirrhotic appendix. We do not call a cirrhotic kidney an indurated kidney, and why should we call an appendix that has become smaller an involuted appendix? The pathologist will tell us that there is an increase in the fibrous tissue and a contraction; consequently, a cirrhotic appendix would be a better name.

Dr. Morris (closing).—As to the normal involution of the appendix, I have one patient who has lost ten pounds in weight. He has distinct gastric symptoms. He has an appendix that is two inches long. I have another patient who has a scar of the appendix. He has had two mild attacks of appendicitis, with adhesions, which will require separation. Most of these patients are nervous; they have minor psychoses, which are traps for the profession. Some of these cases are brought about by the toxemia of intestinal fermentation, and the intestinal fermentation is secondary to normal involution of the appendix. Of course, that requires refined diagnosis. We have lots of nervous disturbances from toxemia rather than direct disturbances from the appendix.

Dr. Noble has pointed out that physicians frequently make a mistake in ascribing an old colitis to appendicitis or to the normal involution of the appendix. I have made this mistake myself. I have had cases of colitis in which I have made all kinds of mistakes, but I have never been tripped up on one of these cases and found an old colitis when I expected to find a normally involuted appendix, because I depend upon palpation. I do not care to operate unless I can palpate the appendix.

Dr. Ill speaks of the cirrhotic appendix and takes exception to my nomenclature. While these appendices are cirrhotic, that does not differentiate them quite well enough. There are scar appendices following acute infection.

# SPLENECTOMY.

INDICATIONS; RESULTS. REPORT OF FIVE SUCCESSFUL CASES.

By W. P. Carr, M.D.,
*Washington, D. C.*

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No operation has been more greatly improved in a short time than splenectomy. Its early history is so interesting in this connection that I quote Dr. Maurice H. Richardson[1] as follows:

"In 1826 Quittenbaum, of Rostock, removed a diseased spleen; the patient, a woman, died in six hours. In 1855 Kuechler, of Darmstadt, removed a spleen for malaria; the patient died of hemorrhage in two hours. The first operation in England, performed by Sir Spencer Wells, in 1865, resulted fatally in six days. The first successful case was that of Péan in 1867."

In 1893 Vulpius[2] collected 121 cases of splenectomy with a mortality of 49.6 per cent., and von Ceci[3] a number of cases with a mortality of 51.6 per cent. In 1900 Bessel-Hagen[4] collected 358 cases with 225 recoveries and 133 deaths, a mortality of 37.2 per cent. According to Bessel-Hagen the mortality previous to 1890 was 42.2 per cent., and from 1890 to 1900, 22.8 per cent.

The operations for this decade were as follows:

[1] Dennis' System of Surgery, vol. iv, p. 376.
[2] München. med. Wochenschr., 1904, li, 116–118.
[3] Loc. cit.
[4] Langenbeck's Archives of Clin. Surg., 1900.

| Disease. | Number of cases. | Died. | Recovered. |
|---|---|---|---|
| Leukemia | 7 | 5 | 2 |
| Wounds | 34 | 14 | 20 |
| Abscess | 4 | 0 | 4 |
| Tuberculosis | 2 | 0 | 2 |
| Echinococcus | 10 | 1 | 9 |
| Infarct with engorgement | 3 | 0 | 3 |
| Sarcoma | 4 | 1 | 3 |
| Benign tumors | 2 | 1 | 1 |
| Twisted pedicle | 14 | 5 | 9 |
| Wandering malarial spleen | 15 | 1 | 14 |
| Wandering spleen, idiopathic hypertrophy | 28 | 2 | 26 |
| Fixed spleen, malarial hypertrophy | 64 | 15 | 49 |
| Fixed spleen, idiopathic hypertrophy | 15 | 2 | 13 |
| Banti's disease (hypertrophy with cirrhosis of liver) | 16 | 3 | 13 |
| Total | 218 | 50 | 168 |

Felix Boyer[1] added to this table 35 cases from 1900 to 1904 with 4 deaths and 1 unknown result—a mortality of 11 per cent. or 14 per cent., according to the termination of the unknown case.

The chief cause of death, according to all reports, has been hemorrhage—hemorrhage from the wound, from the slipping of ligatures, and secondary hemorrhages. Mosler[2] says "when the spleen alone is sick there appears to be a deep-founded alteration of the blood. In many cases of plain hypertrophy, and pseudo-leukemia, and neoplasms there is a hemorrhagic diathesis. Also chronic malarial tumors are accompanied by a hemorrhagic diathesis."

Many of the earlier cases died from sepsis, and the chief reduction in mortality has been due to the elimination of hemorrhage and septic infection.

The next most frequent cause of death seems to have been shock.

H. M. Jordan,[3] in the London *Lancet*, calls attention to the

[1] München. med. Wochenschr., 1904, li, 1116.
[2] Paper read before the Med. Soc. Griefswald, 1898.
[3] Lancet, London, 1898, i, 298-217.

intimate relation of the spleen to the sympathetic and vagi nerves. He believes that pulling upon and cutting the branches of the splenic plexus, particularly those passing from the stomach to the upper part of the spleen, causes profound shock.

He describes a method of partial splenectomy that he has performed successfully upon dogs a number of times, which has for its object not only the saving of part of the spleen tissue, but also the avoidance of injury to these nerves.

It would seem that with the improved technique of modern surgery the chief dangers of splenectomy should be overcome and the operation made a comparatively safe one. But, while much has been done in this direction, there is still great room for improvement.

I make no claim for an original operation, but I believe that if the plan of operation which follows be carefully carried out the chief sources of danger—hemorrhage, shock, and sepsis—will be nearly eliminated.

The position of the patient is important. A large sand-bag should be placed under the back, under the upper end of the spleen, and the foot of the table lowered about six inches. The incision may be median or through the left rectus muscle. I prefer the latter, as it gives better access to the phreno-splenic ligament. The length of incision should be governed by the size of the growth and amount of adhesions; but in every case should be ample, and if necessary a transverse incision may be made from the upper end of the primary wound parallel to and half an inch below the lower border of the ribs. I have not found this necessary in any of my cases, but in very difficult cases it would be a great advantage.

Before attempting to isolate and deliver the spleen a careful examination should be made to determine whether this is possible. In some cases, on account of a hemorrhagic diathesis, dense adhesions, or excessively large, numerous, and thin-walled veins, the operation may have to be abandoned. Such cases, while rare, do occur and have been reported by

the best surgeons. The question of removal or abandonment should be settled before starting profuse hemorrhage that may be difficult or impossible to stop.

Isolation is usually easy, but may be difficult or impossible. The splenophrenic ligament should, as a rule, be first attacked. The operator, covering the spleen with gauze, draws it to the right, while an assistant draws the left lip of the wound to the left; this exposes the bed of the spleen and vault of the diaphragm. The spleen may be firmly held in its bed by atmospheric pressure. This is an important fact to remember. In one case, after trying vainly for five or ten minutes to draw the organ out of its bed, I discovered, accidentally, that by passing my fingers between the diaphragm and the upper and outer surface of the spleen and working them behind it, I admitted air behind it and could then readily lift it from its bed.

When it can be done the splenic ligaments and all vascular adhesions should be doubly ligated in sections and cut between the ligatures; but when this is difficult the ligaments may all be clamped and the blood supply entirely cut off in this way. The spleen may then be removed and the clamps afterward sewed round and removed, as the broad ligament clamps are usually removed in oöphorectomy. All the larger vessels should be separately ligated. For picking up these vessels and for handling the splenic ligaments I have found the ring forceps sponge holders very useful. The smooth ring on these forceps holds the most slippery tissue firmly and without tearing, as the ordinary hemostat or clamp is likely to do.

In difficult cases with extensive adhesions it is possible to grasp all the gastrosplenic ligament between the index and middle fingers of the left hand and apply a long curved rubber-covered clamp. As this ligament contains the chief blood supply of the spleen, the danger of severe hemorrhage will be greatly lessened by securing it first in this manner. The other ligaments may then be clamped or ligated and

cut, after which adhesions may be rapidly separated and the bleeding controlled by gauze packing.

A marked increase in the pulse rate is noticed when the spleen is delivered, or when the gastrohepatic ligament is clamped. This, however, is temporary, and in most cases not alarming, unless the manipulations are prolonged. It is probably due to irritation of branches of the splanchnic nerves, and, consequently, pulling upon the ligaments—especially the gastrosplenic—should be avoided as much as possible, and all manipulations of the splenic bed and ligaments should be as rapid and as gentle as possible. Shock should also be combated by putting the patient in the best possible condition before operating, and by having the free use of salt solution and adrenalin when necessary. The usual measures for preventing infection should, of course, be rigidly observed.

The indications for splenectomy may be stated as follows:

1. Injuries that cannot be safely repaired.

2. All chronic enlargements or hypertrophies of the organ sufficiently great to cause discomfort or produce serious symptoms, except in leucocythemia.

3. Benign tumors and cysts that cannot be readily enucleated or removed by partial splenectomy.

4. Movable spleen, when enlarged or causing distress, especially when the pedicle becomes twisted.

5. Suppurative splenitis, unless the spleen is adherent to the abdominal wall so that the abscess may be safely opened.

6. Malignant disease or tuberculosis, when primary and confined to the spleen, or when secondary or not confined to the spleen, *provided* the other parts involved can also be removed or cured.

Partial splenectomy has only been performed on human beings in a few cases where a portion of the spleen, protruding through an abdominal wound, has been ligated and removed. But Jordan's[1] operations upon dogs have shown the possi-

[1] Loc. cit.

bility of safely controlling hemorrhage from the cut surface of the spleen by a sort of shoemaker's stitch through and through the organ parallel to the cut surface. There seems to be no reason why this operation should not give good results in cases of circumscribed injury or small cysts.

*Repair of injury to the spleen* has been frequently mentioned, and a good many scattering cases are recorded, but they seem to be confined to packing a wound with gauze to prevent bleeding. Ligature or partial splenectomy should be preferred on account of allowing primary closure of the wound.

Splenopexis was performed by Rydygier, in 1895, by stitching the parietal peritoneum to the gastrosplenic omentum and thereby fastening the spleen in a pocket. The patient recovered, and the spleen was still in place three months later. Rydygier and several others, on theoretical grounds, have highly recommended this[1] operation for movable spleen, but, when we remember that movable spleens are invariably enlarged and usually diseased, and the ease with which peritoneal pouches stretch, I cannot feel that there is anything to be gained by this operation.

It can be safely said that there are no deleterious remote effects from removal of the spleen, and that its appreciable functions are fully assumed by a greater development of the red-bone marrow and lymph glands. My first case has been under observation for eight years, and is strong and hearty, and in excellent health. Blood examinations, continued for over a year, showed nothing abnormal. Numerous cases carefully watched, for a long period, by many careful observers, bear out this statement. Therefore, when the spleen is exposed by laparatomy and is found enlarged or diseased, I should strongly advocate its removal rather than any attempt at repair or splenopexis. Wounds of the spleen are dangerous on account of hemorrhage. Slight wounds may bleed but little and require no surgical interference; but when symptoms of sepsis or hemorrhage appear after penetrating wounds

[1] Archiv f. klin. Chir., 1895, vol. i, p. 880.

in this region, a free incision should be made and the organ repaired or removed. If the spleen is found healthy the hemorrhage may be controlled by gauze packing or preferably by a through-and-through shoemaker's stitch of catgut or silk encircling the wounded area. Gunshot wounds may be treated by partial excision when suitably situated, or by gauze packing. In one successful case I packed the wound in the spleen, bringing the end of the gauze out through the bullet hole in the abdominal wall and closed the abdominal incision, which was in the median line.

*Ruptured spleens* are invariably diseased and should be excised. Jordan[1] mentions the fact that while in India he was surprised at the frequent reports of death from ruptured spleen. The rupture was caused in most cases by very slight violence, such as a gentle blow with a walking stick, a sharp push with the hand, or a kick with the naked foot.

Kipling has warned travellers in India against the reckless kicking of natives, and the unpleasant habit they have of dying, after such treatment, of ruptured spleen. Spontaneous rupture, or rupture due to muscular exertion or very slight violence, has occurred in typhoid fever, cholera, and chronic malaria. Osler mentions three such cases in his text-book of medicine. The ease with which enlarged spleens are ruptured, particularly malarial spleens, furnishes a strong argument for their removal.

An enlarged spleen is not only a menace on account of liability to rupture, but in many cases evidently furnishes an altered secretion that is more or less toxic. This secretion appears to be eliminated, in part at least, by the bowel, and acts as an irritant to the intestine, causing, in many cases, severe and intractable diarrhea. Three of my cases had diarrhea that could not be controlled, even temporarily, by rest in bed, strict diet, bismuth, or opium, but in all three cases it ceased at once upon removal of the spleen. In two cases the diarrhea recurred after three or four weeks, but

[1] Loc. cit.

was easily controlled in one case, and in the other was not nearly so bad as before the operation, and could be kept in check by ordinary treatment.

Remarkably good and even brilliant results sometimes follow splenectomy in cases of simple and syphilitic hypertrophy, malarial spleen, Banti's disease, and splenic anemia, while in other cases, apparently similar, little or no benefit follows.

In the present state of our knowledge we cannot always tell certainly which cases will be benefited. In simple hypertrophy, and in syphilitic and malarial spleens, any symptoms referable to the spleen may be expected to disappear after its removal. Complete cure may be predicted in cases of wandering or dislocated spleen and in splenitis. In any case where diarrhea is severe, cure or great amelioration of this symptom may be expected. The majority of cases of splenic anemia are cured by splenectomy.

In Banti's disease, in the earlier stages, arrest or cure of the cirrhotic changes in the liver usually follow removal of the spleen and sometimes a cure is obtained even after cachexia and ascites are marked. In leukemia the operation should never be done, as the mortality is extremely high and no benefit can be expected.

It is obvious that enlarged spleens, except those where removal is contraindicated, should be removed at an early period, when the operation is safe and easy and when the prospects for complete cure are always better. The great advantage of early operation is now generally appreciated in nearly all conditions except enlarged spleen; but it is still common practice to allow spleens to reach a very large size and to wait for cachexia and debility to supervene before operation is thought of. There has been, and still is, too much tendency to look upon all splenic hypertrophy as a result, rather than a cause, of disease. I wish to make a strong protest against this attitude and a plea for early removal of all enlarged spleens beginning to cause symptoms,

unless, as in leukemia, the operation is contraindicated. Appended are my 5 cases of splenectomy: 1 for ruptured malarial spleen; 1 for gunshot wound with uncontrollable hemorrhage; 1 believed to be simple hypertrophy, weighing eight pounds; 1 for Banti's disease, and 1 for hypertrophy of a soft inflammatory nature which we were unable to diagnosticate. The first case was in bad condition from hemorrhage when operated upon, and the last three were all in very bad shape, suffering from cachexia and debility. All recovered from the operation and all were cured, except the last case, whose cachexia continued and caused his death three months later, though for the first month he seemed greatly improved and was entirely relieved of his diarrhea and pain. In the malarial case, the malarial cachexia, which was well marked, disappeared much more rapidly than could have been expected without splenectomy, though he had, of course, proper treatment for that condition.

Case I.—Fred A., white, male, aged eleven years, was brought to the Emergency Hospital, October 28, 1898, after having been kicked in the abdomen by a mule. He had lived along the canal, and had a history of chronic malaria and also showed well-marked yellow skin and emaciation of the malarial cachexia. He was not suffering when he arrived at the hospital, an hour after being kicked, but was too tender over the abdomen to admit of satisfactory examination. His pulse on admission was 110, and two hours later 130, with pallor, thirst, and abdominal distention. He was plainly suffering from intraperitoneal hemorrhage, and exploratory incision was at once made in the median line above the umbilicus. The spleen was found much enlarged and torn nearly in half. It was easily removed, together with about three pints of clots and fluid blood, through the median incision, about four and a half inches long, beginning a half inch below the ensiform cartilage. The vessels were tied with catgut and the wound closed. The boy made an uneventful recovery. Dr. Wallace Johnson examined his blood at inter-

vals of a month for one year. Nothing abnormal was found after the patient recovered from the temporary anemia produced by the severe hemorrhage and splenectomy. This boy called at the Emergency Hospital two years after the operation. He had then grown into a hearty, well-developed lad. I have recently learned that he is still in perfect health and is unusually strong, well-developed, and hearty.

Case II.—Jas. W., male, colored, aged twenty-three years, was brought to the Emergency Hospital, July 4, 1899, suffering from accidental pistol wound of upper abdomen. Condition good; pulse rapidly increased from 110 to 128. Laparotomy was immediately done, and a wound of the spleen near the hilum was discovered which bled profusely. The spleen, which was normal, was easily lifted out of the wound, the pedicle ligated *en masse*, and spleen removed. The splenic artery was then ligated separately. Two or three pints of blood were removed from the abdomen and the wound closed. This man left the hospital in ten days, entirely recovered. I have not seen him since.

Case III.—Mrs. Maria B., female, white, aged forty-four years, was admitted to the George Washington University Hospital, September 6, 1904.

*Family History.* Father and mother died by accident. General health of both good up to time of death.

*Previous History.* Birth and infancy normal. Had measles, whooping-cough, and chicken-pox during childhood; good recovery from all. Menstruation began at seventeen, and was normal until last November, when, at the age of forty-three, it ceased. Gave birth to eleven children, four living; six died during the second summer; one was a premature stillbirth following a fall; had slight lacerations, but has not suffered from womb trouble except from a prolapse that was cured. About nineteen years ago, while living in St. Louis, had malaria, chills, fever, and sweats frequently for about three months. Then came to Washington and had only a few more chills. Has had no chills nor other symptoms

of malaria for eighteen years. Bowels have always been loose. Has had four or five stools a day ever since she can remember. Several times has had diarrhea that prostrated her for three or four weeks. For the last two years has been troubled with palpitation of the heart and shortness of breath on climbing stairs. Recently this has been more pronounced and accompanied by vertigo, making it necessary at times to steady herself by holding on to something. Had bleeding piles for years, but does not suffer from them now.

Has worked in a laundry and in hotel kitchens for several years, doing a good deal of heavy lifting. Often carried heavy weights, resting them on her left side, indicating the left hypochondriac region.

Last March (1904), while stooping over doing some heavy lifting, she felt as if she would "break in twain," and a little later felt a large mass in the left side, which was not painful but tender to the touch. This mass has grown rapidly and now fills the whole left side of the abdomen. For several weeks she has had diarrhea and vomiting. Stools are accompanied by painful cramps and her side hurts her during defecation; also has tenesmus. Has lost fifty pounds in the last year.

*Physical examination* showed her to be sallow and flabby, and hardly able to stand. Pulse 100 to 110, and very feeble. Suspicion of heart (mitral regurgitant) murmur, not sufficiently pronounced to be sure. No enlargement of heart. Abdomen much distended and a tumor distinctly felt, reaching from the left hypochondriac region to the crest of the ilium, and slightly beyond the median line. This tumor had the triangular shape and sharp firm edges of the spleen, and from its situation I had no doubt it was the spleen.

She was put to bed on milk diet and given pepsin and bismuth. Blood examination showed 6,024,000 red and 5200 white cells to the cubic millimeter; no abnormal cells; no malarial organisms; hemoglobin, 70 to 80 per cent. A second count gave 5,932,000 reds and 6550 whites. The

large number of cells was due, I think, to concentration of her blood by the watery diarrhea which she had. The diarrhea continued, with eight or ten stools a day, in spite of bismuth, opium, and diet until the spleen was removed, when it immediately ceased.

*September* 8. Examination of a stool was made by Dr. H. H. Donnelly, who made the following report: Quantity, four ounces; reaction, feebly acid; odor, cadaveric, not fecal; color, grass-green. Considerable mucus intimately mixed and occurring in masses. No blood seen with naked eye. Grass-green watery stool with finely granular green sediment composing two-thirds of the bulk of the stool. White specks (casein) in sediment; pieces resembling fruit skin, 1 to 2 c.c. square and smaller. Tough white coagulated masses (casein? egg?), largest 3 cm. in length, cylindrical, 1 cm. cross section. Micros opic, a good many red blood cells; some leukocytes, some low columnar epithelium; fat droplets; green amorphous material (bile-stained casein); no parasites or ova; vegetable cells.

The urine was normal, except for a very heavy trace of albumin and a large number of leukocytes; no casts.

She was operated on September 15, and the spleen removed through a six-inch incision through the left rectus muscle. The operation was done hurriedly because her pulse was 160 when it was begun. The operation was complete in sixteen minutes and her pulse dropped at once to 140, but came back to 160 for a few minutes. When she reached her room her pulse was 90 and did not get above 104. Owing to the weak pulse no thorough examination was made of the other abdominal organs, but a hasty exploration with the hand revealed nothing. The nausea and diarrhea ceased at once, the wound healed by first intention, and the patient made a rapid and uneventful recovery. She gained rapidly in strength and gained twenty or thirty pounds in weight, judging from appearances, in three weeks, when she was discharged. Three weeks later she returned to the hospital, walking

several squares, and reported herself feeling perfectly well; bowels normal, appetite and strength good.

Blood count two days after operation showed 4,604,00 reds and 10,200 whites; hemoglobin, 60 per cent. to 70 per cent. The diminution in red cells was due, I think, to the stopping of the watery diarrhea which had been concentrating her blood. She lost no blood during the operation. Examination of the urine, six weeks after the operation, showed no albumin, casts, or leukocytes; the urine was normal. When the spleen was cut from its pedicle, a great quantity of blood gushed from it; one and a half pounds of this blood was caught and weighed, and fully as much escaped. The spleen, after this bleeding, weighed four and three-quarters pounds, and before the blood was lost must have weighed about eight pounds. There were several distinct white fibrous nodules showing on its surface, looking like sarcoma to the naked eye. Dr. Carroll, however, examined the spleen microscopically and reports it apparently simple hypertrophy, and the nodules simple fibrous tissue. One of these nodules was so hard as to dull a knife when cutting it.

Case IV.—James C., male, white, aged forty-nine years, farmer.

*Family History.* Father living, aged eighty-six; has always been in good health; mother living, aged seventy-seven; good health. Maternal grandmother died at about ninety, of old age; no history as to other grandparents. Has two brothers and two sisters living; all in good health. One brother was killed by an accident two years ago. No family history of tuberculosis, neurasthenia, hysteria, rheumatism, gout, epilepsy, or insanity.

*Previous History.* Born in Woodville, Virginia. No history as to birth or feeding. Had whooping-cough and mumps during childhood; never had scarlet fever or diphtheria; had measles at nineteen; this left him in a weak and anemic condition. School life lasted from tenth to seventeenth year, but consisted of periods of only a month at a time.

Liked study and made good progress. Worked on a farm the rest of the time. After school worked in a saddle shop four years, and for eight to ten years in a store. Married at twenty-six; wife living and in good health; has one son, age seventeen, in good health.

Drinks four to six cups of coffee daily, on an average; occasionally drinks tea; smokes rarely; uses no drugs; takes an average of one drink of whiskey a month. Believes he has been losing weight gradually, for a number of years; at the age of nineteen weighed 150 pounds; present weight about 130 pounds. Has never had any venereal trouble. Never had typhoid or malaria. Last May he had a severe attack of diarrhea which lasted about a month and greatly weakened him; was not confined to bed, however.

*Present Condition.* At the age of thirty-four patient broke down from over-work, felt very much exhausted, and would often fall down while at work. On February 18, 1905, while performing some heavy manual labor, he felt something "break loose" in the neighborhood of the spleen, which pained him for about a month; a week later he had a fall across the hind gate of a wagon, making it impossible for him to turn over in bed, for six weeks, owing to severe pain. Received medical treatment for a week or so.

Gives a history of occasional attacks of severe weakness, one especially, which occurred about five years ago while he was on horseback; he attributes these attacks to indigestion; does not lose consciousness or get dizzy. Has had diarrhea for several years.

Upon admission to the hospital he was found much emaciated, weak, and with a pulse of 98 running up to 120 on walking across the room. Examination showed great enlargement of the spleen and slight enlargement of the liver.

Operation performed, July 26, 1906, at the George Washington University Hospital. Incision six inches long through the left rectus muscle, enlarged to seven inches. No adhesions were found, but the spleen could not be raised out of its bed.

In attempting to do this, although I thought I was very careful, my fingers suddenly tore into the spleen, making a large ragged hole which bled furiously.

After quickly packing this hole with gauze I worked my fingers behind the spleen and could hear the air rushing in with a hissing sound. The spleen was then readily lifted from its bed, the pedicle grasped and clamped, and the organ removed. The pedicles were then sewed over with catgut, the clamps removed, the larger vessels separately tied, and the wound closed.

The patient made a good recovery, his diarrhea ceasing at once. There were two or three relapses, but these were easily controlled. He left the hospital four weeks after the operation, much improved in strength and general appearance. Several months later he was reported as being in good health.

Microscopic examination by Drs. Grasty and Prentiss showed increase of connective tissue and of Malpighian bodies. I think the case was one of Banti's disease.

Case V.—James A. R., white, male, aged sixty-three years, single, farmer, was admitted to George Washington University Hospital, September 8, 1906. Had "bilious" fever when twelve years old. No serious illness since then until last winter, when present illness began with severe pain in the umbilical region. Pain would come and go, and was a little worse for one or two hours after meals. Has lost twenty pounds. No nausea nor vomiting. Continued to work on the farm during the summer; but the pain grew worse and became localized in the left hypochondriac region. Bowels constipated at first. Of late has had diarrhea. After admission to hospital it was found necessary to give him from two to two and a half grains of morphine sulphate a day to relieve his pain which seemed to be intense. Physical examination showed the spleen to be enlarged to four or five times its normal size, very tender and painful. Other examination negative. Blood count showed 6,150,000 reds and 10,850

whites. No malarial or other organisms. No abnormal blood cells. Urine normal. Examination of stomach contents, after test-meal, showed nothing abnormal. Examination of feces also negative.

*September* 27. His spleen was removed without difficulty. Operation lasted thirty minutes and patient left table in good condition; pulse 108. His pain and diarrhea ceased at once. Diarrhea recurred afterward, but was controllable. He improved greatly during the first month after operation, having no pain, eating and sleeping well. Pain th n began in the umbilical region and grew worse until his death, two months after operation. His death was apparently due to some form of cachexia.

I turned the case over to Dr. E. C. Prentiss, who is making a specialty of stomach diseases. Dr. Prentiss became much interested in the case, and made repeated and exhaustive examinations but was unable to find any definite couse for his pain and exhaustion. He was given injections of trypsin for several days, but no reaction followed. Repeated blood counts showed temporary anemia; 4,500,000 reds, five weeks after operation, being the lowest. Shortly before death the count was 5,600,000 reds; 10,400 whites; hemoglobin, 100 per cent.

No autopsy was allowed; but at the time of operation the other abdominal organs were examined and no disease was found. The spleen, which weighed two and a quarter pounds, appeared intensely red on section and soft in the central portion. It was examined microscopically by Drs. Grasty and Prentiss, who pronounced it a malarial spleen.

## DISCUSSION.

Dr. I. S. Stone, of Washington, D. C.—It seems to me that in the evolution of this operation on the spleen it ought to be possible in certain cases, where great difficulty is expected and where hemorrhage would be fatal, to temporarily secure the

celiac axis before the spleen is attacked. If the tumor is not too large, and there is great danger of hemorrhage, I would like to ask if it is not possible to do the operation in that way? We are perfectly familiar with the location of the arteries in this region, and I do not see why it is not possible to prevent severe hemorrhage if this suggestion is carried out.

Dr. Robert T. Morris, of New York.—I desire to call attention to one point of consequence in operating on these patients, that is, to get ready for intravenous infusion before we operate. Have the cannula ready, and when these patients begin to bleed let the salt solution run. Shock is an important thing. Morphia, I believe, given in advance of the operation, lessens shock. I consider that an important point, although it is disputed by certain therapeutists. Ergot after operation is extremely important.

Dr. Lewis C. Morris, of Birmingham, Ala.—Any one who has had occasion to look up the literature upon surgery of the spleen must be impressed with its sparsity. I have had occasion to look up the literature recently, on account of a case which I have at the present time of congenital enlargement of the spleen. In this case the spleen is so large that it not only fills the upper part of the abdominal cavity, but extends down to the left iliac fossa, going some distance to the right side of the median line. The tumor is a large one.

In considering operation on this case I have decided, since hearing Dr. Crile's paper yesterday, and in view of the fact that the patient is greatly emaciated, that a good thing to do would be for one operator to operate on the spleen while another is making an anastomosis between the bloodvessels of the patient and a donor. If the loss of blood is excessive, fresh blood may be supplied from the donor to take its place. Unless something of this sort is done, the probability is that the patient would not be able to stand the great loss of blood and the shock incident to the operation.

The subject is of great interest to me, and I have been impressed with the scantiness of the literature on it.

Dr. Carr (closing).—In answer to Dr. Stone's question, I recommended an operation in my paper which I do not claim is original with me, but it seems to be the best one I could employ. I did not have time to read the description of the operation. One of the most important things is to get the patient in a proper position, just as you would for an operation on the gall ducts, with a sand bag under the upper part of the spleen and the lower

end of the table lowered. This will bring things into better view in that region. A long incision ought to be made, modified, of course, by the size of the tumor, and, if necessary, a second incision parallel to the ribs. There is no danger of hernia. Hemorrhage from the gastrosplenic ligament, which contains the main blood supply of the spleen, is usually easily controlled. I do not think it would be an easy matter to get at the celiac axis. Of course, the branches are given off in different directions from short stubs, and you could not easily occlude them all.

I do not believe in the use of salt solution before we begin to operate. It is well, however, to have it ready.

Dr. Morris.—But do not let it run until hemorrhage begins.

Dr. Carr.—That is all right. I firmly believe in the use of morphia as it lessens shock both before and after operation. I do not think anything else is as good for that purpose.

# CYSTIC DEGENERATION OF THE KIDNEY.

By J. G. Earnest, M.D.,
*Atlanta, Ga.*

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Some recent works on surgery and practice of medicine do not mention cystic degeneration of the kidney, otherwise known as polycystic disease of the kidney. Others barely allude to the fact that there is such a condition. One gentleman speaks of it as if it was as common as measles, but gives us no specific cases or figures. A prominent London surgeon, writing on the subject the past summer, was only able to cite five authentic cases in adults diagnosticated during the life of the patient. In a rather long experience, I have never seen a case before. I have inquired of several of my professional friends who have had large experience in abdominal surgery, and none of them have ever seen a case. Putting these facts together, I feel justified in stating that it must be a very rare disease in Georgia.

My patient was a woman of about forty-eight years, unusually strong and healthy until about two years ago when she began to have attacks of obstinate constipation that required very drastic remedies for relief, and which always wound up with very large watery stools followed by suppression of urine, more or less complete, and accompanied by severe headaches. A few months before coming to me she noticed a growth in her abdomen. This was found to be gradually increasing in size. She reported to me for examination on February 19, 1906. Her general appearance was good, heart and lungs sound, and the urinalysis showed nothing abnor-

mal. When the abdomen was uncovered, quite a prominent mass was apparent between the pubis and umbilicus. Upon palpation it felt very much like an ovarian tumor, reaching on the right side from the pubis to a point somewhat above the umbilicus. On the opposite side was a mass somewhat smaller and more spherical. I had seen intraligamentous cysts two or three times larger, that by palpation seemed much like these. However, a digital examination per vaginam soon satisfied me that they were not intraligamentous, but I believed they were of ovarian origin as I could distinctly feel the lower end of the right tumor pushing down well over the pelvic brim. The two tumors were in close contact and the area over them more or less resonant on percussion. The fact of this resonance, while not the rule in ovarian cysts, was by no means fatal to such a diagnosis. On the following day, after the usual preparation, I made an abdominal section. When I got in I was quite surprised to find the tumors extraperitoneal. When this was demonstrated I at once suspected that they originated in the kidneys. The tumor on the right side was exposed by lifting the peritoneum and found to be a case of cystic degeneration of the right kidney. The mass was about eight inches long and five and a half in diameter in the centre, and was made up of cysts ranging from a millet seed to a large cherry in size. The vessels and ureter were examined; there was apparently very little normal kidney left. As it was decided not to puncture the cysts, the tumor on the left side, which was nearly spherical and about six inches in its longest diameter, was not disturbed. The abdomen was closed with three rows of buried catgut. The patient rallied promptly. For three days the kidneys acted freely. On the third day she was given a saline which, after being repeated several times, was followed by a high alum enema without any practical result. She then had ten grains of calomel at bedtime. On the following day this began acting. Toward evening, the stools instead of getting less began to get much larger and

exactly resembled the pea-soup actions of Asiatic cholera. She was given two teaspoonfuls of paregoric. After a few hours the actions grew less and finally stopped. It was found that the kidneys had quit acting. We resorted to every known means at our command for restoring their function without effect. She fell into coma and died on the sixth day.

At the operation the question arose as to whether I should puncture the cysts and reduce to a minimum the pressure upon the portion of kidney still sound. I have since learned that Henry Morris did that in one case with marked temporary relief. He also advises that when one kidney is in an advanced stage and the other comparatively small it would be good surgery to remove the one most advanced. These cases are practically always bilateral, and I must say that I can see no benefit that could come from removal of one kidney, and I greatly doubt the propriety of puncturing the cysts. I am much inclined to adopt the dictum of Cheyne and Burghard that these cases are not amenable to surgical treatment. Where the diagnosis is clear, the surgeon should write over them *noli me tangere* and pass on.

The etiology of this trouble seems not to be very clearly made out. However, it is a well-known fact that it occurs much more frequently in infancy than in adult life, and is sometimes fatal in utero, while others born with it survive for several months or occasionally for years. Osler suggests that all cases are congenital. I find some difficulty in reconciling the facts in my case to this theory. I had delivered her, twenty-seven years before this operation, of a large child, and found no evidence of trouble at that time, and feel quite sure the kidneys were not in the same position found at the operation, even if normal in size. I can hardly understand why it should have taken cystic kidneys forty-five years to attain a size sufficient to attract attention, when in the past year she thought they had doubled in size.

# REPORT OF A CASE OF RUPTURED OVARIAN TUMOR WITH COMPLICATIONS.

By Rufus B. Hall, M.D.,
*Cincinnati.*

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The writer knows full well that to come before this Association with a report of a single case would be a breach of professional courtesy, if there were not unusual conditions to justify the time required in doing so. He also realizes, that it is in the study of individual cases with complications that one profits more than in the tabulating of large numbers of ordinary uncomplicated cases, and this is his apology for taking up your time in presenting the short report of this case.

Mrs. R., aged fifty-two years, mother of two children, the youngest twenty years of age, passed her menopause four or five years ago without any unusual complications. For a year or more, just preceding her operation, she complained of vague stomach derangement complicated with what the physician in charge termed gastralgia and indigestion. This condition did not improve much under his careful medication. She had bad spells occasionally and would then visit her doctor, but was able to superintend her household duties. About the first of the year, she took suddenly ill with all the symptoms usually attending intestinal obstruction, excepting she did not have the stercoraceous vomit. These acute symptoms, lasting thirty or forty hours, were finally overcome by her family physician by high rectal enemata and small doses of calomel. In a few days she was as com-

fortable as ever, with the usual stomach symptoms remaining. April 18 she was again seized with the acute pain in the abdomen, with vomiting, and grew gradually worse until the evening of the 22d, at which time her abdomen was moderately distended by gas and all the symptoms of intestinal obstruction, at which time she was seen by the writer. Her temperature was 102.5°, pulse 155, the face was pinched, extremities cold, with regurgitating vomit every few minutes, principally of the water which she insisted upon drinking. Physical examination revealed the fact that she had a tumor in her abdomen, and from the condition present the diagnosis was made of ruptured ovarian cyst probably associated with peritonitis, and an immediate operation was advised. After a family consultation and much time wasted, the patient was moved to my private hospital and an operation was made early on the morning of the 23d. At the time the patient was given ether her pulse was 168, temperature 103°, and respiration 52. The abdomen was moderately enlarged from the distention of the intestines by gas. When the abdomen was opened it was found that she had a ruptured ovarian cyst. The cyst before rupture was about the size of an adult's head. It was a papilloma of the ovary. The fluid in the abdomen was very thick and ropy and very tenacious.

Evidently she had the rupture for at least six days, from the time of the commencement of her illness. There was general peritonitis; every portion of the peritoneum was very red.

After bailing out as much fluid as we could hurriedly, the tumor was ligated and removed. On the opposite side was a second tumor about the size of the closed hand, adherent in the pelvis behind the uterus. This was removed and proved to be a papilloma also. We spent several minutes' valuable time trying to wipe out the sticky, viscid ovarian fluid which adhered to every portion of the intestinal tract

and peritoneum, and when we were about ready to commence closing the abdomen I discovered that she had an enlarged gall-bladder, which was much larger than the closed hand, with a gallstone blocking the cystic duct.

I asked my anesthetizer if she could stand a few more minutes on the operating table. His reply was that she had not had any ether since the abdomen was opened, that her pulse could be felt but not counted, and he thought she could stand a few more minutes. I decided to remove the gallstones through a second incision over the gall-bladder. After a short incision made, and a portion of the gall-bladder pushed out through the incision, the gall-bladder was aspirated and fourteen ounces of fluid removed, also forty-two gallstones about the size of the end of the little finger. The gall-bladder was stitched to the abdominal wall and a rubber drainage tube was placed in the gall-bladder. The original abdominal incision was closed, a glass drainage tube placed, and the patient put to bed. She rallied slowly and made a very satisfactory convalescence without a single complication in any way until the eighteenth day after her operation. The drainage tube in the abdomen was removed on the third day and the entire wound closed without a drop of pus. The drainage tube in the gall-bladder was removed on the ninth day, and on the fourteenth bile ceased to pass and none passed afterward. On the eighteenth day after her operation, May 11, she was seized with the most excruciating pain on the right side of the abdomen in the region of the appendix, but a little lower down in the pelvis, the pain extending down inside of the thigh. The pain was so severe that she cried out from it, and I was first inclined to believe that something had gone wrong about the catgut used on the pedicle and made a vaginal examination to determine this, but the examination was negative. She grew worse, and within a half hour her pain was so severe that we were compelled to give her hypodermics of morphine. In the first ten hours she required four doses

of $\frac{1}{4}$ grain each to make her condition tolerable. After that $\frac{1}{4}$ of a grain once in five or six hours was all that was required. The pain continued for twenty-six hours when it suddenly disappeared. The pain was so much like nephretic colic that I had the urine examined frequently for pus and blood. Excepting a few pus cells, the examination threw no light upon the diagnosis.

The right half of the abdomen was so sensitive to pressure and the muscles so rigid that a bi-manual examination was not very satisfactory, yet the day her pain subsided I felt reasonably certain that she had an enlarged kidney and so stated to her physician. Fourteen days later, on May 25, she had a recurrence of a similar attack, the pain lasting for five or six hours, requiring three doses of morphine, but she had been able to sit up for a few days before this time, and after the pain subsided a few days she went home, on May 30, without a positive diagnosis as to the cause of the pain having been made. She gradually improved in strength and flesh without a recurrence of the pain for six weeks, at which time, July 5, she commenced to complain of a discomfort in her abdomen located principally in the right side. She sent for her physician, who could discover nothing abnormal upon examination. The patient was a small woman, weighing not more than 115 pounds. The discomfort gradually increased. Her physician saw her on the 9th, making daily visits thereafter until the 13th, when at that visit he discovered that she had an enlargement in the right half of her abdomen.

This enlargement was the size of a quart cup. I was asked to see her, which I did on the 14th. Upon examination I found an elastic tumor, as above described, occupying the right half of the abdomen and extending back to the loin, but not extending into the pelvis. The fact that she had a ruptured papillomatus tumor removed on April 23 made us naturally consider malignant disease, but the fact that this was fluid, apparently in a cyst and not free in

the peritoneal cavity, knowing that both ovaries had been removed, made the diagnosis obscure. She was suffering considerable pain and experiencing great difficulty in getting the bowels to move. We advised aspiration of the tumor with the view of clearing up the diagnosis. This was done, and two and one-third pints of fluid removed that was amber in color, devoid of smell, and the tumor entirely disappeared. This fluid was turned over to a pathologist, who was asked to make a report as to the nature of it, telling him that we believed that it was urine. He reported that it positively was not urine and he did not believe it was ordinary ascitic fluid. The tumor reappeared and by the evening of the 17th, in just three days, it was more than twice as large as it was before it was tapped. By this time she had developed all the symptoms of acute intestinal obstruction. She had been urged to have an exploratory operation made before this, as she was growing worse, but she and her family were so disappointed that she should so soon have to submit to another operation that they would not consent until the 18th, when her symptoms became so desperate that they consented, and she was sent to the Bethesda Hospital and an immediate operation was made. At this time the tumor appeared almost as large as an adult's head. One could detect fluctuation over the whole surface of the tumor.

The patient was *in extremis*. She had taken scarcely any food for more than a week, her pulse was 168 and temperature 100, and she had regurgitating vomit every few minutes. The abdomen was opened along the outer border of the right rectus muscle for three and a half inches in length over the tumor. When the abdomen was opened it was found that there was a sharp kink in the small intestine just as it entered the colon, by doubling it over on itself. The large intestine was collapsed, while the small intestine was distended by gas and liquid feces. The tumor was clearly post-peritoneal. It was aspirated through the incision, external to the large intestine, and two and two-

third quarts of slightly amber-colored fluid removed and the tumor disappeared. This at once corrected the kink in the intestine and feces and gas commenced to pass into the large intestine. It was so evident now that it was an extravasation of urine that I at once hurriedly passed a sharp-pointed pair of scissors through the opening made in the peritoneum by the aspirator, out through the loin just below the kidney, and placed a rubber drainage tube. I closed the opening in the peritoneum and then closed the abdomen as rapidly as possible. It was believed that the patient would succumb on the table, but she did not. She rallied very slowly. Excepting the fistulous opening, she made a good recovery and remained well so far as her intestinal obstruction was concerned. She left the hospital for home in three weeks.

After knowing the condition we had to deal with I tried to locate, by palpation, a stone or the cause of the extravasation of the urine, but I could not do so, and the exact location of the rupture remained a mystery. After three or four days, when convalescence had been established, by collecting the urine which she passed from the bladder and the urine from the tube in the loin, the amount from each kidney did not vary half an ounce in a given number of, say, six hours. My past experience, amounting to two cases, previously reported to this Association at its Louisville meeting in 1905, of injured ureter with post-peritoneal accumulation of urine drained through the loin, led me to hope that nature might be able to relieve the obstruction in the ureter and thus re-establish the communication with the bladder. Therefore I advised her to wait the results a few months, which she did until September 17, when she entered my private hospital for the purpose of having the kidney removed, which was done on the 18th. She recovered promptly from the operation, relieved of her fistulous opening which had annoyed her very much. She has since regained her usual health.

I realize that objection may be raised to sacrificing the kidney, and at the same time advocates making an effort to locate the rupture and repair it, rather than to sacrifice the kidney; but the fact that she had a perfectly normal kidney on the opposite side, and the great difficulty of locating the injury and the uncertainty of being able to repair it, plus the danger of fatal termination, were all discussed and considered by both the writer and the patient, and she chose the latter operation. I am convinced that it was a wise choice, and I report the case as illustrating the many unlooked-for complications, necessitating severe operations for relief, arising in one patient in such a short time.

One of the most interesting points in connection with the case is, What was the cause of the post-peritoneal extravasation of urine? From the clinical history one would be inclined to believe that the patient had an impacted, irregular-shaped stone in the ureter since her first severe attack of pain, which came on so suddenly while she was in the hospital on May 11. We assume that it was irregular in shape because she was perfectly relieved for fourteen days, when she had the second attack which lasted six days, when she was relieved for a period of six weeks, when in all probability, due to the ulcerating process, the ureter gave way and the extravasation occurred. If this surmise is correct I do not know what became of the stone. It was not detected during the operation or since.

I have purposely omitted making a long and detailed report, preferring to give the main facts in the case so as not to weary you by details with which you are all familiar.

## DISCUSSION.

Dr. John B. Deaver, of Philadelphia.—Dr. Hall is to be congratulated on the favorable outcome in this case. It is a remarkable one both from the standpoint of recovery of the patient and the pathology. Retroperitoneal extravasation of urine is a rare condition. I have practised surgery for a good many years and have yet to see such a case. I believe there was an ulceration produced by the small stone which was probably not found at the time of the evacuation. The recovery of the patient shows not only the wisdom but, as said before, the good judgment of the operator in pursuing the course he did when confronted with such a case.

Only comparatively recently I removed a kidney where there was complete occlusion of the ureter by a small calculus at about two inches distant from the pelvis of the kidney. I took out the kidney. A pathological examination of the specimen was made by Dr. A. O. J. Kelly, who pronounced it a typical carcinoma of the renal pelvis and ureter. The patient made a good recovery.

# HODGKIN'S DISEASE A TYPE OF SARCOMA.

By William B. Coley, M.D.,
*New York.*

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That both pathologists and clinicians at the present time are widely at variance as to what constitutes Hodgkin's disease is a proposition that, I believe, will be readily admitted at the outset.

Martin, professor of medicine, McGill University, writing upon this subject, uses the designation "Lymphosarcomatosis," thus showing that he inclines to the malignant theory of the disease. In his conclusions he states that the term "Hodgkin's disease" is a purely clinical one and is associated with very different pathological conditions, the two main varieties being the granulomatous and lymphomatous.

Two years later Simmons,[3] in a paper upon Hodgkin's disease, in giving the pathological analysis of nine cases made at the laboratories of the Massachusetts General Hospital for the Harvard Medical School, concludes that Hodgkin's disease should be regarded as an entity, presenting a definite histological picture.

In one of the most recent papers on the subject, giving the results of the *x*-ray treatment in Hodgkin's disease at Bruns' Clinic, Tübingen, the condition is described as malignant lymphoma.

The pathologist's view of Hodgkin's disease has been, perhaps, best described in the now classical paper of Dorothy Reed,[1] on the "Pathological Changes of Hodgkin's Disease, with Special Reference to its Relation to Tuberculosis."

Although the paper contains a very careful review of the literature of the subject, its chief value lies in the fact that it represents the most careful and painstaking study of eight cases of Hodgkin's disease, observed at the Johns Hopkins Hospital, that has yet been made. The main points of pathological significance are described as enlargement of lymph glands, absence of infiltration of the capsule, grayish surface broken by intersecting yellowish lines, metastases of the viscera presenting a similar appearance, though of irregular outline and apparently invading the organ in different directions. On section they present the same appearance as described in the glands. On microscopic examination there is found dilatation of the bloodvessels and lymph sinuses, marked increase in lymphocytes and proliferating endothelial cells, numerous giant cells with one or more nuclei, and prominent nuclei appear in great number; occasionally a very large giant cell with many small nuclei peripherally arranged is seen. An increased number of eosinophiles is generally observed. Reed states that the capsules of the glands usually show no change except, possibly, some thickening. This, however, is not borne out by other writers, especially by Gibbons in his recent work.[4] Careful autopsy studies were made in five of his cases, while in Johns Hopkins Hospital the material was limited to three autopsies only.

The large giant cells are the most striking feature of the sections from Hodgkin's disease tumors. Reed states that they occur in great numbers in the large lymph sinuses of the glands, and occasionally are seen in the bloodvessels. They vary remarkably in size, from two to three times the size of a red blood corpuscle to twenty times its size. Sometimes as many as eight to ten nuclei are seen in a single cell. Large numbers of eosinophiles were observed in all but two of Reed's cases.

The clinical picture of Hodgkin's disease is as follows:

Hodgkin's disease almost always begins in the glands of

the neck, may occur at almost any period of life but is more commonly seen at an early age, and is more frequent in the male than in the female. A tubercular family history is rarely found. The glands begin to enlarge on one side first; very quickly enlargement is noticed on the other side also; a little later the axillary glands are affected, and finally the inguinal glands, etc., too, become involved. These enlarged glands differ clinically from both ordinary sarcoma of the lymphatic glands and tubercular adenitis. On palpation they show much greater mobility than is seen in sarcoma or tubercular glands, giving the sense of a discrete nodule. The skin is not affected; the glands move freely upon the deeper parts. There is entire absence of the "fusing together" in these independent masses, so commonly found in tubercular adenitis. Neither is there any tendency to breaking down or caseation. Later on anemia develops without any particular increase in leukocytes. The spleen is often markedly enlarged, and, in the later stages, also the liver, and marked cachexia appears. In some cases the spleen extends nearly down to the brim of the pelvis (see photo).

Although during the last twenty years numerous writers have attempted to maintain the tuberculous origin of Hodgkin's disease, the most elaborate paper holding this view being that of Sternberg,[5] this theory has been so thoroughly controverted by more recent writers, particularly by Reed, that it can claim no further consideration. Reed admits only two etiological possibilities: one, that Hodgkin's disease is a malignant growth; the other, that it is an infectious process. She believes that clinical and histological evidence is in favor of the theory of its infectious rather than malignant origin. As in favor of the infectious theory she cites the following facts: First, that fever is frequently associated with Hodgkin's disease. My observation has been that it is also not infrequently found in malignant disease, particularly sarcoma, in which generalization has taken place.

Second, that the frequency with which the disease starts in the cervical region suggests the possibility of infection gaining entrance through the mucous membrane of the mouth. The same reasoning would apply to cases of sarcoma of the neck, which comprise such a large portion of the total number of sarcoma. My own statistics show 74 cases of sarcoma of the neck in a total of 615 cases. Then, again, Reed states the growths differ from malignant tumors by the absence of capsular infiltration and implication of adjacent tissues, declaring that sarcoma does not confine its growth to gradual extension from gland to gland. Exception must here be taken to the premises, inasmuch as Gibbons' cases have shown that capsular infiltration does take place and infiltration of neighboring tissues, even of bone tissue, may occur in Hodgkin's disease.

Still another objection to the malignant theory, and the one most frequently advanced, is that metastases occur only in pre-existing lymphoid tissues. This would, indeed, be very strong evidence against the malignant theory, if it were true; but the cases of Gibbons, together with the observations of Banti,[6] the distinguished Florentine pathologist, furnish conclusive proof that metastases in Hodgkin's disease may occur in locations where there is absolutely no lymphoid tissue present. The 5 cases in which Gibbons was able to carefully study the autopsy findings all showed lymph-gland involvement all over the body, with metastases in the liver in 4, in the spleen in 4, in the kidney in 2, in the lungs, pericardium, and pancreas 1 each. In 2 there was a large adherent tumor mass of the neck which involved the surrounding structures, fascia and muscles, and in 1 part of the cervical vertebra and wall of the jugular vein was involved.

Now, we may have a tumor starting in the glands of the neck, resembling the clinical picture of Hodgkin's disease, that later invades the axillary glands, glands in the groin, or retroperitoneal glands, finally causing death, and yet

repeated examinations by the most skilled pathologists will report the tumors round-celled sarcoma (see Cases II and III below).

Again, we may have tumors originating in the inguinal glands, later involving the abdominal glands, that clinically have the appearance of typical cases of sarcoma, and pathologists who examine the specimens may pronounce it Hodgkin's disease, as is well illustrated by the following case:

CASE I. *Sarcoma of the Inguinal Glands, Simulating Hodgkin's Disease.*—H. M., aged thirty-seven years, was admitted to the General Memorial Hospital, August 24, 1906. The patient was referred to me by Dr. A. G. Gerster as a case of inoperable sarcoma. Family history negative. The patient stated that he had always been in good health. Three years ago he first noticed a small swelling in the right groin. This grew slowly until it reached the size of a walnut; was never painful and general health remained perfect. Five months prior to his admission the lumps in the groin began to increase in number and to grow rapidly in size. They finally interfered with his walking.

Physical examination at the time of his entrance to the hospital showed heart and lungs normal. His right inguinal region is occupied by a number of independent tumors, more or less closely fused and extending deeply into the iliac fossa. The skin over the growths was freely movable. The right thigh and leg were considerably swollen. Inasmuch as the tumor mass seemed so unusually movable, it was deemed wise to attempt removal. This was done by my associate, Dr. Downes, September 4, 1906. An incision, nine inches long, was made from the anterior superior spine passing over the middle of Poupart's ligament and down along the course of the femoral vessels, and a very large number of nodules varying in size from a marble to a lemon, all more or less completely surrounded by a capsule, were removed. The peritoneum was opened accidentally

in one place and closed with catgut sutures. The wound healed satisfactorily without suppuration. Microscopic examination of the growth made by Dr. Clark, assistant pathologist of the hospital, and confirmed by Dr. Wood, of the College of Physicians and Surgeons' Laboratory, pronounced it Hodgkin's disease. Portions of the tumor were also examined at Cornell laboratory and the same diagnosis was made. Blood examination on May 7 showed: red cells, 4,200,000; white cells, 51,000; polymorphous, 35 per cent.; lymphocytes, 65 per cent.; hemoglobin, 80.

After the wound had healed, the nuclei-proteid serum from a case of Hodgkin's disease, prepared by Dr. S. P. Beebe from the Huntington Laboratory Fund for Cancer Research, was begun. Nine tubes of 15 c.c. each were given without apparent effect. October 16, the serum was given up and the patient was put upon the mixed toxins of erysipelas and bacillus prodigiosus. Up to the present time nineteen injections have been given in doses of 0.5 mm. to 13 mm. On October 16, the right thigh measured eighteen and one-quarter inches, being two inches larger than the left. Physical examination at that time showed the right inguinal and iliac regions occupied by a tumor which apparently infiltrated the adjacent structures, as its limits could not be well defined. In the right hypochondriac region, just to the right of the median line, there was a hard mass evidently attached to the spine and extending from the median line nearly over to the lumbar region and up almost to a level with the right costal arch. No enlargement of the spleen and liver could be detected. During the past month, under the toxin treatment, there has been some improvement in the abdominal condition; the tumor in the iliac fossa is somewhat smaller and the mass in the right hypochondrium is not nearly so pronounced. Measurements of the leg remain the same.

The blood examinations made in this case during November and December, 1906, showed the following:

| *Red blood cells.* | *Hemoglobin.* | *White blood cells.* | *Poly.* | *Lymph.* |
|---|---|---|---|---|
| 4,200,000 | 80 | 51,000 | 65 | 33 |
| 4,000,000 | 75 | 34,800 | 64 | 34 |
| 3,800,000 | 75 | 38,000 | 55 | 45 |
| 3,600,000 | 70 | 56,000 | 60 | 40 |
| 3,400,000 | 65 | 56,000 | 65 | 35 |

The principal reason for reporting this case is that it emphasizes, more clearly than any case I have seen, the striking similarity between Hodgkin's disease and sarcoma.[1]

This case and a number of others somewhat similar which have come under my observation have strengthened the opinion I have long held, that Hodgkin's disease is really a variety of sarcoma rather than an independent disease.

CASE II. *Extensive Recurrent Round-celled Sarcoma of the Cervical Region. The later history almost identical with that of a case of Hodgkin's disease.*—Miss M. A., aged forty-five years, first noticed a tumor just above the clavicle about two years ago. This was removed. Shortly after this a recurrence was noted in the right cervical region, which grew with great rapidity, very quickly involving the axillary glands on the same side. In July, 1900, the axillary tumor was removed by Dr. C. B. Nancrede, of Ann Arbor, Michigan. The tumor in the cervical region showed such extensive involvement of the deeper structures that it was considered entirely inoperable. Microscopic examination was made by Dr. King, of the Dartmouth Medical School, who pronounced it small round-celled sarcoma. The patient was referred to me by Dr. Nancrede for the toxin treatment, in October, 1900. Physical examinations at this time showed a large tumor in the right cervical region, extending along from the

[1] The patient died at King's County Hospital, Brooklyn, in March, 1907. The important point in the autopsy was a further confirmation of the opinion of Gibbons that the tumors of Hodgkin's disease may infiltrate other tissues. In the present case "the mass of tumor involved the whole of the psoas muscle on either side and was continuous with the periosteum of the vertebral bodies." A section cut out of the bodies of the second and third lumbar vertebræ showed the tumor had involved the vertebræ themselves as well as the periosteum.

clavicle to the mastoid process of the temporal bone. There were some enlarged nodules below the clavicle, and several small tumors in the region of the cicatrix in the axilla. The toxins were begun at the General Memorial Hospital, October 1, 1900, and continued, with intervals of rest, for six months. There was a very decided decrease in the size of the tumors and increase in their mobility. This continued for about three months, and then, in spite of further injections, the improvement ceased.

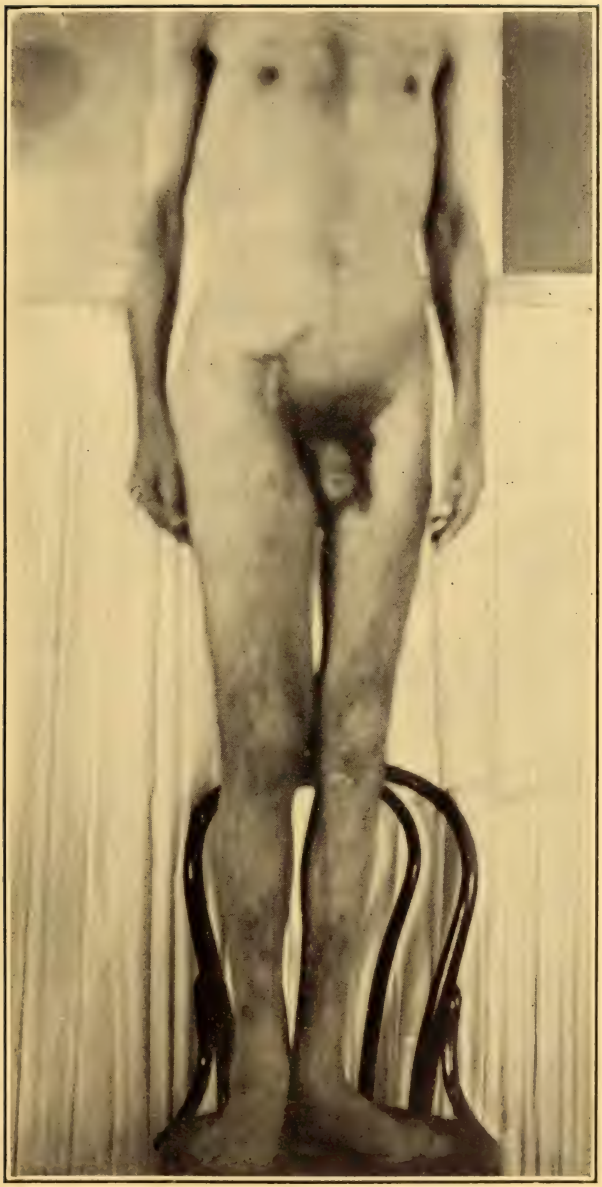
The tumor masses, globular in outline, more or less discrete in character, gradually involved the outer side of the neck as well as both axillæ. The tumors gave the appearance of encapsulation. The photographs will give a good idea of the condition prior to 1902 when the *x*-ray treatment was begun. The result of the *x*-ray treatment in this case was the most remarkable I have ever seen. The tumors decreased in size with great rapidity and at the end of four months had almost completely disappeared. A few months later, however, the disease recurred in both groins and in the mesenteric or retroperitoneal glands. The *x*-ray treatment had apparently lost its influence and toward the end an enormous number of subcutaneous growths appeared in all parts of the body; seven hundred of them, varying in size from that of a pea to an English walnut, were present at the time of her death, in June, 1904.

Case III. *Small Round-celled Sarcoma of the Neck, Six Times Recurrent.*—G. F. H., aged seventy years. About thirteen years ago a lump was first noticed in the left side of the neck. Nine years ago this was removed at the New York Hospital. The tumor recurred one year later in the scar, and grew much larger. The patient has had four subsequent operations within the last seven years. One tumor was removed from the right side of the neck, the others recurring in the region of the cicatrix of the original growth on the left side and behind the ear. The pathological records of the New York Hospital show the following:

"February 21, 1892. G. F. H., aged sixty years, has had a tumor on the left side of the neck, posterior to the scalenus anticus muscle, for three years and four months. Of slow growth at first, the tumor has increased in size rapidly during the last three months. No glands were involved. Operation February 20, 1892. The specimen is an oval tumor, 7 x 4½ inches; no capsule; on surface the voluntary muscle fibers are seen invaded by the tumor tissue at several points. Microscopically, the tumor is a typical round-celled sarcoma with small amount of fibrous-tissue stroma, apparently derived from tissues in which the growth originated."

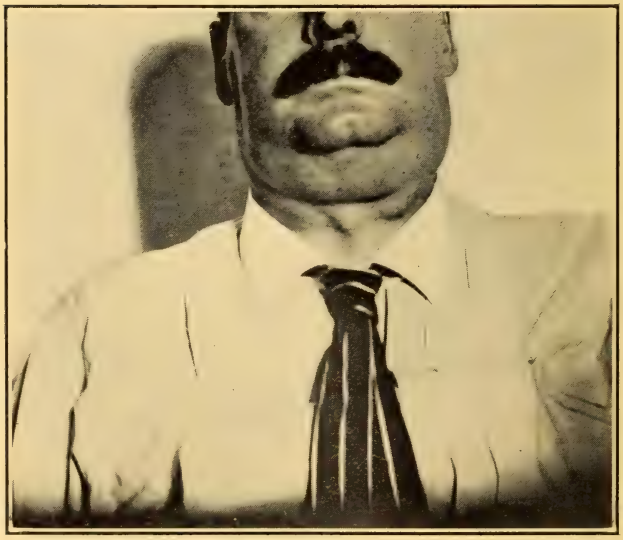
On May 13, 1901, the patient was referred to me by Dr. E. M. Foote, of the Vanderbilt Clinic, as a case of inoperable sarcoma of the neck, for treatment with the erysipelas toxins.

Physical examination at that time showed a tumor the size of an orange occupying the left mastoid region. The tumor was very vascular and semifluctuating. There was a glandular involvement behind the jaw on the right side; also a growth the size of a small orange in the right femoral region. There was a hard, freely movable glandular tumor in the right axilla the size of an English walnut. The patient was put upon the erysipelas toxins, but derived little or no benefit from the same. He returned to his home in Brooklyn, and later came under the care of Dr. E. R. Fiske, who started the *x*-ray treatment in December, 1901. The influence of the *x*-ray in this case was very remarkable. Almost immediate improvement followed; the tumors began to soften and gradually disappeared entirely. The patient was shown at the *x*-ray meeting of the Academy of Medicine in March, 1902, in perfect health, without the slightest suspicion of a tumor in the neck. The growth in the right axilla had also disappeared. The patient soon after developed similar tumors in both groins, which nearly disappeared under renewed *x*-ray treatment. He remained well until the summer of 1903, when he began to complain of rheumatism,



Case I

Case II



Case IV*a*

Case IV*b*.—Showing enlarged spleen in Hodgkin's disease.

Case IV*c*.—After one month's treatment with *x*-ray and mixed toxins.

Case IV*d*.—After two months' treatment.

Case V

as his physician wrote me, and died on September 23, 1903, under symptoms of cerebral thrombosis.

There is very little doubt in this case that the patient died of general metastases.

Here we have a round-celled sarcoma starting in the neck, later involving the axillary glands, still later the inguinal glands and finally destroying life by internal metastases, almost a typical picture of the course of a case of Hodgkin's disease, and yielding to the influence of the *x*-ray, precisely as do many cases of Hodgkin's disease.

The following is one of the most typical cases of Hodgkin's disease that have come under my observation:

Case IV.—W. V. B., aged forty-three years, married, twenty-five years. Family history negative. Operation for mastoiditis in 1901; in 1899 he had some trouble with the molar tooth in the left lower jaw. In the spring of 1902, he first noticed a slightly swollen gland in the left submaxillary region, following a severe cold. This slightly increased in size; almost simultaneously a similar swelling developed on the right side of the neck. Gradually the number of enlarged glands increased on both sides of the neck and submental region. A few weeks after that he noticed enlarged glands also in the axilla and still later in both groins; this was soon followed by enlargement of the abdomen. The patient was referred to me by Dr. Hollister and admitted to the General Memorial Hospital, September 25, 1902. Examination of blood prior to admission showed 65,000 leukocytes and 7,000,000 red blood cells.

Physical examination at the time of admission to the hospital showed the patient somewhat emaciated. There was a large glandular tumor occupying both cervical and submental regions, extending down to the clavicle. The glands in the axillæ were enlarged to the size of an English walnut. Both groins were filled with large masses of glands varying in size from a walnut to a hen's egg. On examination of the abdomen, a hard, immovable tumor was found 8 x

11 inches, of apparently splenic origin, extending from the left side to within two inches of the right anterior superior spine and within one inch of the symphysis pubis. The patient was put upon the *x*-ray and toxin treatment. The first two weeks he was given small doses of arsenic, which were later discontinued. The *x*-rays were given four times a week, ten minutes' exposure in the face and ten minutes in the abdomen. Blood count was made every week, the principal feature of the counts being that the red cells were nearly normal, very slight leukocytosis, but a marked decrease in polynuclear cells.

Blood count, October 4, 1902: hemoglobin, 60 to 70 per cent.; leukocytes, 11,000; differential polynuclear, 7.7 per cent.; lymphocytes, 91 per cent. (142 counts).

Blood count, October 13, 1902: hemoglobin, 70 per cent.; leukocytes, 464,800; red blood cells, 92; differential polynuclear, 6 per cent.; lymphocytes, 92.3 per cent.; eosinophiles, 8 per cent.; mast cells, 4.

Blood count, October 22, 1902: hemoglobin, 60 to 70 per cent.; leukocytes, 9000; red blood cells, 3,600,000; differential polynuclear, 18 per cent.; lymphocytes, 81 per cent. (202 counts).

Blood count, October 29, 1902: lymphocytes, 81 per cent.; polynuclear, 17.4 per cent.; eosinophiles, 4 per cent.; mast cells, 9 per cent.; hemoglobin, 50 to 60 per cent.; red blood cells, 4,500,000; leukocytes, 6000.

Blood count, January 7, 1903: red bood cells, 3,807,000; leukocytes, 2750; mononuclear, 48 per cent.; polynuclear, 52 per cent.; eosinophiles, none seen; hemoglobin, 62 per cent.

While at first there was marked improvement under the treatment, the disease soon got the ascendancy again, and the patient died in May, 1903.

Case V. *Round-celled Sarcoma of the Supraclavicular Region.*—I. L., male, aged thirteen years. Family history good; present history, received an injury above the right

clavicle three months before the tumor was noticed. The growth increased rapidly in size. The patient was operated upon on July 29, 1905, by Dr. Gerster, of the Mt. Sinai Hospital. The outer third of the clavicle was removed. The tumor was densely adherent to the periosteum, but the bone itself was not involved. On February 3, 1906, the patient was referred to me by Dr. Gerster, with a large recurrent growth at the site of the original tumor. Physical examination at this time showed several more or less distinct masses in the right supraclavicular space, extending down to the upper pectoral region; axillary glands not involved. The patient was put upon the mixed toxins, the injections being made in the pectoral region three or four times a week. Slow but steady improvement occurred. There was gradual diminution in size and increased mobility of the tumors. The toxins were kept up in large doses, the injections being made partly in the tumor and partly in the pectoral region during the entire spring and summer, four to five times a week. In June he was given *x*-ray treatment for three weeks. In the early part of July the tumors had almost entirely disappeared and his general condition was good. He was sent home, but came back in September with a marked return of the trouble; the tumors had again reached almost their original size. He was again put upon the toxins, but with practically no effect in retarding the growth of the disease. The accompanying illustrations show the condition at this time. Loss of weight has been rapid. Blood examination shows: leukocytes, 19,700; polymorphonuclear cells, 70 per cent.; large mononuclear, 2; small mononuclear, 9; red blood cells, 6,100,000; hemoglobin, 50 per cent.

Blood count made November 27, 1906, shows the following: red blood cells, 4,200,000; hemoglobin, 80; white blood cells, 12,400; polynuclear, 60 per cent.; lymphocytes, 33; eosinophiles, 5 per cent.

Blood count, January 12, 1907: red blood cells, 4,200,000;

hemoglobin, 60 per cent.; white blood cells, 18,000; polymorphonuclear, 43; large lymphocytes, 7; small lymphocytes, 31; eosin, 19; blastomyces, 5.

The last examination, as will be seen, shows an enormous increase in eosinophiles.

During the last two months he has developed similar tumors in the axillary region and in both groins. As yet no internal metastases can be made out.

I have had under observation five other cases in which the clinical picture was that of Hodgkin's disease, but no microscopical examinations were made of the tumors.

In regard to the value of blood examinations in the diagnosis of Hodgkin's disease, opinions vary. Reed and most writers believe that it offers nothing characteristic, although often there is an increase in the number of eosinophiles.

Cabot, in a series of 32 cases, found distinct lymphocytosis in 11; Da Costa, in 10 cases, found lymphocytosis in 3; Longcope found lymphocytosis in only 1 of 8 cases; Stengel, of Philadelphia, considers it rare; while Pincus, of Berlin, regards it as present in the majority of cases. Several of my cases show it in a marked degree.

I have had blood examinations made in a large number of ordinary types of sarcoma, giving results almost identical with those found in Hodgkin's disease. The majority of cases showed nothing particularly characteristic, with the exception of a moderately increased number of leukocytes. A certain number of cases showed distinct eosinophilia and a marked lymphocytosis.

Let us examine, for a moment, the chief points upon which the pathologist bases the diagnosis: Marked stress is laid upon the encapsulation of the tumors and absence of infiltration; the fact that only pre-existing glands or lymphoid tissue, as has been stated, are invaded; peculiar types of giant cells. Yet all of these characteristics may be found in certain types of sarcoma.

Those who, like myself, hold the opinion that Hodgkin's

disease is a variety of saroma, are not really so far away from the position held by Reed and others, who claim it to be of infectious origin, for I have long believed and endeavored to show that sarcoma is itself due to some form of micro-organism which has the nature of an infectious process. A careful clinical study of my own cases of sarcoma of the neck, axilla, and groin has caused me to believe that the so-called Hodgkin's disease is but a variety of sarcoma, often sufficiently distinct to present a fairly characteristic clinical picture, and at other times so closely resembling the more common varieties of sarcoma, both clinically and microscopically, that it cannot be differentiated. Most important of all is the fact that this variety goes on and destroys life in precisely the same way and in about the same length of time as sarcoma. It forms metastases in the liver, spleen, and other organs, and fully deserves the name of a malignant tumor. Beebe and Ewing's experiments with sarcoma in dogs, at the laboratory of the Huntington Cancer Research Fund, have proven that these tumors, until recently believed to be infectious granulomata, are actually sarcomas. When we have discovered the cause of these tumors, it is possible, as I have stated many years ago, that we may find different, slightly varying forms of microörganisms of some general type that give us the varying types of malignant tumors. Or, perhaps, according to the opinion of Ballance, of England, some one organism under different conditions produces different types of tumors, just as the same insect in vegetable life, under different conditions, will produce entirely different types of galls upon trees.

The favorable action of the *x*-rays upon Hodgkin's disease might be offered as an objection to regarding such tumors as sarcoma. It is true that a very much larger number of cases of Hodgkin's disease disappear or show improvement under the *x*-ray treatment than in the ordinary type of sarcoma, but if the later history of these cases is traced the end results will be found to be practically the same.

Pfeiffer[7] has recently reported the cases of Hodgkin's disease treated with the *x*-rays at the Tübingen Clinic, and has likewise collected the end results in all the cases that have thus far been published.

They apparently believe in the sarcomatous nature of those tumors at the Tübingen Clinic, inasmuch as these cases are reported under the name of "Malignant Lymphoma."

Pfeiffer collected 33 cases in all, including 7 observed at Bruns' Clinic. Of these 10 were observed but a short time; 14 cases from one to nine months; 9 cases were observed over nine months. His summary of the 33 cases shows that 7 died, 2 without any noticeable change in the tumor; in 5, or 15 per cent., there was no apparent improvement; 28 showed local or general improvement. There were but 9 cases in which the period of observation extended over nine months; of these 2 showed recurrence. Only 3 of the entire 33 patients were known to be alive nine months and over: namely 1 nine months, 1 eleven months, and 1 eighteen months.

One of my own cases of Hodgkin's disease treated with the *x*-ray, quoted by Pfeiffer, while much improved at the end of four months, began to get worse shortly afterwards and died, with cachexia and jaundice, within eight months.

The strongest argument in favor of regarding not only Hodgkin's disease, but leukemia as well, as a form of sarcomatosis, has just been brought out by Banti *(loc. cit.)* He believes that both the lymphatic and myelogenous types of leukemia are really malignant neoplasms of the sarcomatous type. He states that on the sarcoma hypothesis all the hitherto unexplainable pathological and clinical facts are readily accounted for, and he concludes:

Lymphatic leukemia is a unique disease; it is always alike in its nature and is a special form of sarcomatosis. Myelogenous leukemia he believes to be a systemic myeloid sarcomatosis of the hematopoietic and lymphopoietic organs,

the so-called myelocytes being really neoplastic cells which have entered the circulation.

My own conclusion, based upon a study of 615 cases of sarcoma, personally observed, 74 originating in the lymph glands of the neck, is that Hodgkin's disease is merely a special variety of sarcoma, maintaining as a rule a fairly definite clinical and pathological type, but yet, in many cases, shading off into other types that correspond most closely with the ordinary types of round-celled sarcoma. The very evidence brought forward by Reed and others in favor of the infectious origin of Hodgkin's disease need not be disproved. Most of it holds true of sarcoma, and all of it but adds to the slowly accumulating mass of evidence in favor of the microparasitic or infectious nature of sarcoma as well as carcinoma.

The question of retaining the name of Hodgkin's disease is not of great importance compared with the recognition of the true pathological condition, just as it matters little whether we call a certain disease of the thyroid gland exophthalmic goitre or Graves' disease. If the malignant nature of the disease becomes generally accepted, as I believe it undoubtedly will, it will be hard to find a better name for the disease than lymphosarcoma, which Virchow applied to it many years ago, prefixing multiple to it, or calling it lymphosarcomatosis to distinguish it from the sarcomata confined to a single lymph gland.

Bibliography.

1. Reed, Dorothy. Johns Hopkins Medical Reports, 1902, vol. x, p. 133.

2. Martin, Chas. F. Journal of Medical Research, July, 1901, p. 249.

3. Simmons, Channing C. Journal of Medical Research, June, 1903, p. 378.

4. Gibbons, Henry Walter. Amer. Jour. Med. Sci., November, 1906.

5. Sternberg. Zeitschrift f. Heilkunde, 1898, Band xix, p. 21.

6. Banti. International Clinics, vol. iii, series xvi.

7. Pfeiffer. Bruns' Clinic, Tübingen. Beiträge z. klin. Chir., 1906, Band l, 263.

# INJURIES AND DISEASES OF THE HYOID BONE.

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The hyoid bone is a small structure, and is situated in such a position as to be well protected from injury, yet one is surprised to find so little space devoted to a study of its diseases and injuries. A careful search of the literature will show only a moderate consideration of the fractures of this bone, and almost none of its other pathological conditions. *Fractures* of the hyoid bone are of very exceptional occurrence and are usually produced by a direct blow or fall, or by lateral compression, as by the pressure of an adversary's hand on the throat, and in a few cases by muscular contraction. It is also well known that the bone is sometimes fractured by judicial, but not by suicidal, hanging. The fracture is usually that of one of the greater cornua, but occasionally the body of the bone has been broken. Fracture of both cornua has been observed. As the bone is usually broken by throttling, the right cornu is the part most often fractured, owing to the pressure of the right thumb of the assailant.

The symptoms are generally quite characteristic: the patient usually feels a distinct snap, or sensation of a solid body giving way in the upper part of the neck, with severe pain, difficulty in speaking, swallowing, or even in breathing, and cough. Increased mobility of the parts may be elicited and sometimes crepitus. The fragments may be driven inward and perforate the mucous membrane of the pharynx and cause bleeding from the mouth. Swelling and ecchymo-

sis may occur externally, and a finger introduced into the pharynx may feel the ends of the broken bone. The injury is serious from its accompanying complications and death has followed in several instances; but if life is preserved, bony union usually takes place. I have a well-marked example of fracture of the hyoid bone with bony repair in my collection, but do not know the history of the case as the specimen was obtained from a cadaver. (See Fig. 1.) The treatment is directed more toward relieving the threatening symptoms than toward the fracture. If there is severe dyspnea, a tracheotomy may be required; if dysphagia, the patient must be fed for many days through a tube or by rectal enemata. Silence must be enjoined and all movements

FIG. 1.—Fracture of great cornu of hyoid bone.

of the tongue prohibited. The fragments may be replaced by pressure with the finger from within the pharynx, and counter pressure from without, and the head should be immobilized in the extended or flexed position, as is most comfortable to the patient and most effective in preserving reduction. If apposition cannot be maintained, I do not see any objection to suturing the fragments together. At times an abscess forms and a sequestrum may be thrown off; occasionally the voice is changed for a long time or permanently.

*Inflammation* of the hyoid bone may result from external violence or constitutional disease, and usually begins as a

periostitis, with localized pain, swelling, dysphagia, and dyspnea. Suppuration generally occurs, and if the pus is not promptly evacuated it may burrow in many directions and cause extensive havoc. Necrosis of the bone is likely to occur in such conditions, usually limited in extent, though the whole bone has been known to die and be cast off. After the extrusion of the sequestrum, the functions of the bone appear to be but little affected.

*Neoplasms* of the hyoid bone are of great infrequency and I have only been able to find five or six cases reported in the literature that is accessible to me. In most of the works on surgery which I have been able to consult no mention is made of tumors of the hyoid bone at all, and the references to the subject in the literature are exceedingly sparse. A writer in von Bergmann's *System of Surgery* (American edition, 1904, p. 38) says that only two cases of primary tumor of the hyoid bone are recorded in all literature. The occurrence of neoplasms of the hyoid bone, both primary and secondary, is without doubt of great rarity, but I have been able to collect five or six cases, inclusive of one of my own, which I will report briefly at this time.

Case I.—*Enchondroma of the hyoid bone*, reported by E. Boekel in the *Gazette de Strasbourg*, in 1862, which the author remarks "is unique in literature." "A woman, aged fifty years, observed in 1859 a tumor on the right side of the throat, which gradually increased in size. The neoplasm was the size of two fists, fluctuating, and hard at its base, like cartilage. It raised up the inner head of the sternocleidomastoid and pressed against the larnyx, then extended under the chin and terminated in a blunt point 2 cm. below the sternoclavicular articulation. It was somewhat movable and the skin was not adherent to the growth. The thyroid gland was small and not involved. Swallowing solids was very difficult, but respiration was not impeded. On puncture of the tumor several grams of liquid jelly escaped. There was no pain. Extirpation was done and the growth was

found to arise from the horn of the hyoid bone, which was resected and the tumor easily removed. There was very slight bleeding. The growth was 12 cm. long and 7 cm. thick. It rose from the periosteum of the horn of the hyoid bone, and consisted of hyaline cartilage with a great number of cells. The patient did well until the fourth day, when secondary hemorrhage set in and, notwithstanding the ligature of both the external and later common carotid artery, she died of anemia and exhaustion." This case is quoted by Dr. J. Spisharny, of Moscow, in the *Deutsche medicinische Wochenschrift*, vol. xviii, p. 853.

Case II.—*Tumor springing from the horn of the hyoid bone, causing suffocation through lateral compression of the epiglottis.* "On November 5, 1867, a man aged twenty-three years appeared, of fair and delicate complexion, who spoke in a very guttural manner, as if his mouth was full of food. Six months previously his voice had become affected, commencing with a cold and sore throat. A swelling formed on the left side of the neck which affected speech and swallowing, with dyspnea at times, especially at night, and sometimes a little cough. He could swallow solids and liquids, but as the tumor increased in size this varied, and he became thinner. His previous health had been excellent. He became very weak, with a feeble pulse. The fauces appeared healthy. By laryngoscopic examination a rounded tumor, the size of a large walnut, was seen on the left side of the throat at the root of the tongue and pushing the epiglottis laterally, so that it looked as if folded in two. The entrance to the larnyx was obstructed. The tumor appeared somewhat ulcerated, and was hard but not painful. In the neck, externally, there was a swelling above the thyroid cartilage and connected with the left horn of the hyoid bone. It had increased in size somewhat rapidly of late. No operation was done and the man returned home, where he died suddenly on November 9, 1867. The growth was supposed to be malignant, but no microscopic examination was made." Reported by Sir G.

Duncan Gibb in the *Transactions of the Pathological Society of London,* 1868, vol. xix, p. 59.

Case III.—*Primary enchondroma springing from the great horn of the hyoid bone; excision and recovery.* "On March 22, 1891, a man, aged twenty-five years, came under observation. On the right side of the throat immediately below the lower jaw, in the location of the hyoid bone, was a tumor the size and form of a hen's egg. The skin was not altered and was movable. The tumor was rough, hard, and but slightly movable, and was connected with the right horn of the hyoid bone and followed the movements of this bone. It was not painful. The thyroid cartilage was pushed to the left of the middle line. The orifice of the larynx could not be seen, but a lump the size of a walnut could be felt at the base of the tongue, which was rough, hard, and but little movable, and was connected with the growth in the throat. The corresponding tonsil was swollen. A laryngoscopic examination was impossible. Swallowing solids was difficult, the voice hoarse, and respiration labored when he was on his back. The respirations were 26 to the minute. The patient was well nourished and the other organs normal. Eleven years ago he noticed that his voice was hoarse. No lump was observed at that time, but five years later the swelling was noticed and grew slowly. Five months before coming to the clinic he began to have dysphagia and marked hoarseness of voice. On April 10, 1891, under narcosis, Professor Sklifosowsky operated in the following manner: An incision was made at the level of the angle of the mouth and extended in a curved manner to the cricoid, the sternomastoid and vessels pulled outward, and the growth enucleated, when it was seen that it had developed from the right horn of the hyoid bone. Releasing the growth was not easy, and was accomplished by blunt dissection with the index finger. The bleeding was slight. Only the facial, lingual, and a few small vessels were ligated. The right horn was excised at its junction with the body of the bone. He was discharged

well in a month. After the operation the temperature remained normal, the voice became clear, and the breathing and swallowing perfect. The tumor was irrugularly oval in shape, length 7 cm., breadth 6 cm., and thickness 4.5 cm. The structure was mostly hyaline cartilage, rich in cells, and the tumor surrounded with a connective tissue capsule." Reported by Dr. J. Spisharny, *Deutsche med. Wochenschrift*, 1892, vol. xviii, p. 853.

Case IV.—*Mixed round and spindle-celled sarcoma of the hyoid bone; recovery.* The patient was a negro, aged twenty-four years; married; waiter. No family history of malignancy but of tuberculosis. He had had gonorrhea, but not syphilis. There was a lump the size of an English walnut beneath the chin. The skin was stretched and ulcerated at one point; the larynx was not involved. The tumor is away from the median line, and upon the right greater cornu of the hyoid bone. It has been growing for five months, and has been painful one month, and is now very tender. Deglutition and respiration are interfered with. Chloroform anesthesia. Operation on April 7, 1898. Dr. Dawbarn excised the left external carotid artery. Ten days later the right carotid was excised and the growth ablated. The tumor was encapsulated and easily removed. It was attached to the greater horn of the hyoid bone, and one-half of the hyoid was removed. The mouth was not opened nor was the thyrohyoid membrane torn. Some suppuration occurred, but the man recovered and resumed work. Prize essay by Dr. R. H. Dawbarn, *The Treatment of Certain Malignant Growths by Excision of the External Carotids*, p. 33.

Case V.—*Sarcoma of the hyoid bone and larynx, with excision of the tumor of the hyoid, base of the tongue, larynx, and part of the pharynx, under local anesthesia.* On January 9, 1906, J. W., aged forty-five years, white, tailor by occupation, was admitted to the University Hospital, having been sent in from the throat and nose dispensary, where he was examined by Professor John R. Winslow. He was at that

time suffering from dyspnea, due to a growth in the region of the hyoid bone, which so pressed upon the epiglottis and larynx as to produce difficult respiration and to prevent a laryngoscopic examination of the air passages. The diagnosis was tumor of the hyoid bone involving the larynx. The patient is married and has four healthy children. His parents lived to a good old age, and he does not know the cause of death of either of them. He has had the usual diseases of childhood, as well as smallpox, and sixteen years ago had a sore on the penis with suppurating inguinal glands, but this was not followed by any secondary symptoms. Thirteen years ago he noticed a small lump in the left side of the neck, which was excised by Professor W. W. Keen at the Jefferson Medical College Hospital, Philadelphia. Through the kindness of Professor Keen the following history has been obtained:

"J. W., aged thirty-four years, was admitted to the Jefferson Hospital on March 31, 1893; discharged April 6, 1893. He had a tumor in the right side of the neck the size of a hulled walnut. He complained of no pain or any other symptom. The presence of the tumor was the only physical sign. There was no family history of tuberculosis or malignant disease. The tumor was removed by Dr. W. W. Keen, April 1, 1893. He found it attached to the hyoid bone and the side of the larynx. Five days later the patient left the hospital with the wound entirely healed."

No pathological examination of the growth was recorded. This history is probably erroneous in stating the tumor to have been located on the right side, as there was a well-defined scar on the left side of the neck and none on the right, and the patient said the growth was on the left side. He had been a regular drinker, but, he said, never to excess. After the removal of the growth mentioned above, he enjoyed good health for three years, when he noticed a recurrence, and it has been increasing in size slowly ever since. About five weeks before admission to hospital, the respiration began to

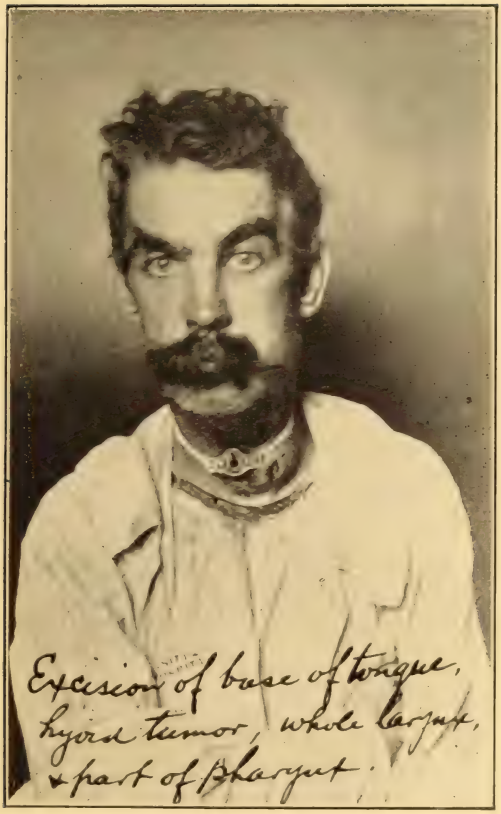
Excision of base of tongue,
hyoid tumor, whole larynx,
+ part of Pharynx.

FIG. 2

be interfered with and there was also some difficulty in deglutition. His voice is altered and he has some cough. He is pale, emaciated, and weak. There is marked dyspnea, with stridor, which makes an examination of the chest difficult. The heart is exceedingly feeble and beats from 110 to 140 times a minute. The apex beat is not visible or palpable, but the heart-sounds are clear and the second aortic sound is accentuated. The abdomen presents nothing of importance. There is a large swelling occupying the location of the hyoid bone and extending more towards the left than the right side of the neck. This enlargement is globular, as large as a goose's egg, hard, and freely movable. It is not painful but causes discomfort. The Adam's apple can be seen and felt below the tumor, and the larynx is not enlarged or displaced. A skiagraphic picture fails to show the hyoid bone, but a shadow of a growth is faintly seen in the area between the jaw and the larynx.

On January 11 his respiration had become so impeded that immediate relief was demanded, and a laryngotomy in the cricothyroid space was done under cocaine. This relieved his dyspnea and rendered him much more comfortable. He was desirous of having the growth removed, but his condition did not justify such a serious procedure. He was, therefore, put on tonics with digitalis, fed well, and his pulse gradually increased in strength and diminished in frequency, ranging from 70 to 100 per minute, but still of very poor volume. As he still wished to be operated on it was thought unwise to place him under a general anesthetic; hence he was given 1 ounce of whisky every hour by mouth, from 3 to 8 A.M., and morphine, grain $\frac{1}{4}$, and scopolamine, grain $\frac{1}{100}$, one-half hour previous to operation, and infiltration anesthesia of the skin with Schleich's solution was effected. On February 1, 1906, he was properly prepared, and while still awake the operation was undertaken and completed. The tumor of the hyoid was first removed with a part of the base of the tongue, when it was seen that the larynx

was also involved. The incision was, therefore, extended downward in the middle line, the skin reflected, and the whole larynx easily removed without hemorrhage. The end of the trachea was brought out at a lower level and sutured to the skin. A large gap was left, leaving a wide opening into the pharynx. The pharynx was sutured to the base of the tongue and closed in the middle line so as to make a funnel-shaped canal, and the skin was loosely sutured. The patient stood the operation very well; complained of but little pain, and was in as good condition at its close as at its beginning. He would not have survived a general anesthetic. He was returned to bed and put in an almost upright position to facilitate the swallowing of the saliva, and a tracheal tube was kept in the trachea. At first a large soft-rubber catheter was passed from the mouth into the esophagus, and water and milk allowed to run into the stomach, but subsequently the lower part of the pharynx opened and the tube was passed into the esophagus from the neck. There was a free discharge of saliva from the opening in the neck, which was kept from entering the trachea by wrapping the tracheal tube with gauze. The incisions healed promptly except a small place at the bottom, and the patient gained in strength. Three quarts of milk, with eggs and whisky, were administered daily. He soon sat up and walked around the ward, and in a measure enjoyed life, but in the latter part of March there was a recurrence in the lower portion of the neck, and he gradually failed, and died on March 27.

The tumor was a round-celled sarcoma. A few injections of Coley's mixed toxines were given, as well as *x*-ray treatments, without benefit. The autopsy showed extensive metastases in the lungs, bronchial glands, liver, and mesenteric glands, as well as in the tissues of the neck. I regard this as a case of primary sarcoma of the hyoid bone, subsequently extending to the larynx, but of this there is doubt. Professor Hirsh, who examined the tumor of the hyoid, not finding any myeloid cells, is inclined to think the starting

point was in the epiglottis. The hyoid bone was completely destroyed by the growth, only some calcareous particles remaining, while the epiglottis was not entirely destroyed, nor was the larynx as much involved as one would expect if the growth had originated in the epiglottis. The patient said the growth began to recur three years after the removal of the tumor by Dr. Keen, approximately ten years ago.

CASE VI.—*Osteoma of the right horn of the hyoid bone.* Reported by Dr. John C. Warren, *Surgical Observations on Tumors*, p. 117. The patient was a man who applied to his father, Dr. John Warren, on account of a lump in his neck. This was a conical exostosis of the right horn of the hyoid bone, nearly three inches in length. This was excised and the man recovered.

Even secondary growths of the hyoid are of great rarity; but Spisharny reports one case, in which Peter found a metastasis in the body of the bone, at the autopsy of a person who died of cancer of the esophagus.

Tumors of the hyoid bone, as far as they have been reported, have been either sarcoma, enchondroma, or osteoma, and doubtless this bone is subject to all the neoplasms that are found in the other bones of the body, though with greater infrequency.

In the cases tabulated the ages varied from twenty-three to fifty years of age, and five of the six occurred in men. The length of time intervening between the first observation of the tumor and the removal of the growth, or death of the patient, varied from five months to ten years. The growth also varied in size, having been the size of two fists in an enchondroma, goose's egg, hen's egg, conical osteoma three inches in length, and an English walnut.

Deglutition was affected generally, and respiration more or less embarrassed. The voice was altered in nearly all cases, and pain or discomfort was generally present. Cough was also present in some cases. Of the six cases five were subjected to operation, with four operative recoveries and

one death from secondary hemorrhage. One case succumbed eleven weeks after the operation from local and general metastases, and one died suddenly without operation, probably asphyxia, as the larynx was compressed by the growth.

The treatment of tumors of the hyoid bone, whether benign or malignant, should be thorough removal at as early a period as possible, with as much of the contiguous tissues as may be necessary. The extirpation of this bone, even when it becomes necessary to excise the larynx and other contiguous tissues, is not attended with extraordinary danger, and the possibility of a permanent cure in malignant cases depends upon an early and radical operation.

# SOME DISJOINTED OBSERVATIONS UPON OUR TECHNIQUE.

By A. M. Cartledge, M.D.,
*Louisville, Ky.*

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For the past few meetings of this and some other societies I have been somewhat disappointed on finding little or nothing about our old friend, technique. Several years ago it was different; like water in the little song, it was "Technique, technique everywhere."

Well, it is a good thing to exchange recipes every now and then; to know how the boys are washing their hands or whether they are washing them at all; whether they are wearing gloves and if so what kind, and a hundred other little manners of the surgical kitchen.

While the current literature is always more or less full of improved points in technique, I learned years ago that this, like most subjects, gives its best to discussion among the men who work. I am sorry I have nothing new to offer you; no additions. My few remarks will probably partake more of the destructionist than of the constructionist; but, seriously, I believe anyone who can demonstrate satisfactorily how to simplify our technique by the omission of any detail, has added greatly to its structure. I have always considered the after-management of abdominal operations a very important part of the technique, and so it is. You had a little free-for-all, go-as-you-please, at that yesterday. The old-time hair pulling we used to have over these points did much to relieve us as well as to impress the young in the field.

During the years, each of us in turn has evolved from his

daily task, somewhat, a practice, a technique of his own. If it has proven satisfactory in meeting the requirements of modern aseptic surgery, and is less cumbersome or in any way more desirable than usual ways, then he has a right to speak and be jumped upon in the proper baptismal way. I am still washing my hands—in fact, believe that I do it better every year—and think that you do, or did, but without knowing it; hence, when gloves came about, you adopted them.

A few words about gloves:

That the hands are the most important feature connected with sterilization there can be no question, as it is universally recognized that infection through the hands is the chief source of infection at the time of operation.

I am not opposed to gloves as a general proposition, but I do claim that they have objections as well as advantages. In cases where the all-important sense of touch, in deep and inaccessible parts, where touch alone is vitally important, they are a distinct disadvantage. Again, in rapid and neat work they are a drawback. Really, I think we have not attached enough importance to the question of infecting our own hands while we were paying all attention to the question of infecting the patient. I wear gloves in infectious cases to protect my next patient. In clean cases I make everybody else wear them, but believe that I make my own hands sterile and am able to do better work without gloves. A glove is a great thing to the surgeon in septic cases, and a great thing for all assistants and nurses at all times.

As to the method of cleaning hands, I have nothing new to offer. I simply advise lopping off what I consider some unnecessary, hence bad, technique. Green soap and water with brush, making the test when the hands get to the condition of the washerwoman, then immersing the hands in alcohol.

As to sutures and ligatures, I am using more catgut and the Pagenstecher linen instead of silk.

There is one feature in the technique of a few surgeons (and formerly of many) to which I desire to call attention in these desultory remarks.

Do you still use iodoform? Gentlemen, I sincerely believe that if you fully appreciated how objectionable you are to decent and refined people when saturated with iodoform, that you would strive your utmost to do away with this, to my mind, worthless smeller. In common with the majority of our profession, I employed it for years and thought it absolutely necessary to my success. I never use it or any substitute now, and can truthfully (but not tearfully) say, "I miss you."

Yesterday Dr. Boldt, in his remarks before the Society, touched upon the question of purgatives before and after operation. I feel that this is too important a part of our technique to pass over. While the liberal movement of the bowels previous to operation is essential, our former system of purging the patient with salines until he could scarcely stand up, was a very bad one, tending to make the life of the patient subsequent to operation worse than miserable by adding to postoperative thirst. This, however, is a slight harm compared to the common practice of purging people violently the second and third day after abdominal operations. The patient was supposed to be empty previous to the operation, has had nothing to eat for two days, and then another purgative! This of itself is enough to make many people ill. A gentle enema the second or third day, repeated if necessary, is all quite sufficient, and in many cases I do not give a cathartic until the fifth or sixth day. Of course, this does not apply to cases of great distention, or where there happens to be indications for a cathartic.

*Scopolamine-morphine.* While I have never used this as an anesthetic, and do not very well see how others can get their consent to rely upon it for that purpose; as a preliminary to ether I find it, except in the old and those with a kidney

lesion, an excellent thing. The patient has less nausea, less pain, and less mucus in the throat; all very desirable.

Now I have offered you nothing new. I simply want to add my experience in favor of simplicity in technique and reliance upon things that are tried and true, always remembering that the most complicated and so-called highest order of technique alone cannot give good results in surgery.

Our most famous quartette in surgical technique, at least up to the present time, are heat, soap, salt, and water.

# THE REPORT OF A CASE OF AËROGENES CAPSULATUS INFECTION.

By Andrew J. Coley, M.D.,
*Alexander City, Ala.*

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In reporting this case it is not with the intention of opening up a new field in medicine or surgery, but to help in keeping constantly before the mind of the medical world such diseases as are virulent and yield little to treatment and are, comparatively speaking, seldom seen by the majority of the profession.

It was not my good fortune to make a bacterial study of this case, owing to the inconveniences connected with a country doctor's life. The symptoms were so urgent and acute in this case that a diagnosis was easily made. I will not undertake in this paper to describe the germ, its nature, nor its habitat. I might say, however, that this germ was described and named by Welch, of Baltimore, in 1892. It has been so thoroughly studied and described by him and other eminent bacteriologists that it would be entirely out of place in an Association, composed of so many physicians and surgeons of wide reputation, for me to undertake so great a task.

Mrs. B., a widow, mother of four children, aged thirty-five years, white, was of healthy parentage; father died at seventy-two of pneumonia, mother died at seventy-six of cholera morbus. This woman had always been healthy and strong until twelve months prior to this illness, at which time she had a chronic diarrhea which emaciated her very much. I was called in to see her, when, passing by, for the purpose of

extracting a tooth which was loosened almost sufficiently from pyorrhea to drop from her gums. I was immediately impressed with her condition by the excruciating pains of which she complained and the anxious expression.

The extreme pain together with her badly amaciated condition presented to me a patient whom I knew was strangely and unusually ill. The condition of the gum and the surrounding parts was not significant of any very serious trouble—the tooth was easily removed with scarcely any pain or hemorrhage. The suspicious condition of the patient impressed me, and caused me to advise perfect cleanliness and precautions, by using frequent mouth washings with a normal salt solution. This tooth was the first molar of the left lower jaw. I remained with the patient fully a half hour and I left her, I thought, in as good condition as was possible under the circumstances. I did not hear any more from this patient until two days later when I received a summons hurrying me back to see her. On my arrival I found her with a pulse of 120; temperature, 101°; respiration, 26; and facial expression indicating great pain and discomfort. She had had no distinct chill but complained of frequent chilly sensations. On examining the gum from which the tooth had been extracted it presented nothing of very unusual significance other than a slight swelling and some induration. I cleansed the cavity thoroughly with a bichloride solution, 1 to 1000; there was a marked tenderness in the neighborhood of where the tooth was extracted. The pain was so excruciating that it was necessary to administer a hypodermic of morphine. I advised a continuation of the same plan of treatment as first prescribed, together with a nutricious diet consisting of eggs, beef-juice, etc. On my return next day I found the patient still complaining of very acute, heavy, aching pains all over that side of the face and under the jaw. After making a very careful examination I found, on deep palpation, slight fluctuation. I advised an emoluent poultice to be kept up constantly until my return the next day, at which time the

fluctuation was more marked and I could feel a cracking sensation under the skin overlying the diseased parts. Under local anesthesia I made a free incision, from which poured a very foul-smelling pus loaded with air bubbles.

It was very evident to me what my diagnosis should be—aërogenes capsulatus infection. I washed out the cavity thoroughly and frequently with a strong bichloride solution, 1 to 2000, and advised a continuation of the prescribed plan of treatment—hypodermics, nourishing diet, etc.—for the next twenty-four hours, at which time I would see the patient again. This visit, which was about the sixth day, found her suffering intensely from the same acute, heavy, aching pains (as she described them).

The induration by this time had extended the full length of her neck and upon the side of her head above the ear, with a number of points of the same peculiar cracking fluctuation, which were each freely opened and irrigated with peroxide of hydrogen. The patient by this time had become considerably weaker, pulse and temperature remained about the same, but she had grown much more restless, so much so that more frequent and larger hypodermics of morphine had to be administered. I found her again the next day in a more critical condition; pulse had become more accelerated (then about 140); temperature slightly more elevated, 101.3°.

Destruction by this time was more manifest, extending all over that side of the face, head, and neck. The whole of this area was freely opened, irrigated frequently, and drained. It was very evident that, with these frequent irrigations and drainings, the germ was very virulent in its nature and was rapidly undermining and destroying my patient.

The next day when I returned I found her in a semiconscious condition, with the evidence that the disease was rapidly spreading over the whole of the head, neck, and shoulders. Notwithstanding the large and frequent hypodermics which had been given she was unable to sleep. The pain was growing more and more intense, and she more

restless, presenting every evidence to me that death was inevitable. Although I made free incisions in all the parts needing them and washed them out thoroughly from time to time with peroxide of hydrogen, the infection still progressed rapidly. In a few hours death came to her relief.

In the beginning of this paper I said I did not hope to add any idea especially new to the infection produced by this germ; but I do want to call the attention of the medical world to the great pain and restlessness from which this patient suffered, and another thing, with all this destruction of tissue, there was but little redness or discoloration of the infected area. During the whole of her illness there was great tenderness to the slightest touch. This led me to believe how little evidence there is in any diseased part or organ inflammation, that we should under all circumstances, where there is much pain, look carefully for the cause and try to relieve it, for the gravity of no disease can be measured by the local manifestation, such as heat, pain, swelling, discoloration, etc.

# SOMETHING ON GASTRO-ENTEROSTOMY.

By Herman Tuholske, M.D.,
*St. Louis.*

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Stomach surgery was born twenty-five years ago. I saw the first resection of the stomach for carcinoma by the great Billroth in June, 1881. The first resection for ulcer of the stomach was made by Rydygier and the first gastro-enterostomy by Woelfler in the same year. For the first five to eight years stomach surgery, on trial, was done with misgivings, watched with disapproval, and adjudged a bold surgical exercise without permanent beneficial results. Today gastro-enterostomy, performed for definite indications and by fairly matured methods, forms one of the most brilliant chapters in the advance of surgery. This short paper aims only at the consideration of some moot points in connection with gastro-enterostomy in cases pathologically benign, but lethal, clinically, under certain conditions. Ulcers of the stomach and duodenum and their sequelæ are the most frequent causes of chronic digestive disturbances, and according to the well-supported statement of Professor Welch, 5 per cent. of all people are afflicted with it. At the Surgical Congress at Wiesbaden, Leube stated, as the result of his enormous experience, that of his 424 patients, three-fourths recovered after four or five weeks' treatment; that of the remainder, one-fifth were improved; only 1.5 per cent. were unrelieved and 2.4 per cent. died. Were this the actual state of affairs, we should be forced to consider ulcer of the stomach a rather benign affair and leave its treatment entirely to the skilled internist. Professor Leube's

statement, however, leaves out of consideration the later fate of his patients, as to recurrence and sequelæ. This is about as fair as if the surgeon operating for cancer of the breast should report the immediate operative results as a criterion of the percentage of cures. Hardly anybody dies of the operation; the percentage of recurrences puts the question in its true light. Internists, fortunately, have given us statistics from the standpoint of later and lasting results. Among these, those of J. C. Warren, of Boston, and of Schultz, of Breslau, are the most important. According to Warren, definite healing occurred in 34.4 per cent.; recurrence in 43.2 per cent.; change into carcinoma in 2.4 per cent.; pyloric obstruction and dilatation of the stomach in 10.4 per cent.; death from perforation in 4.8 per cent.; death from hemorrhage in 4.8 per cent. Hence permanent and temporary benefits altogether, 77.6 per cent.; definite failures in 22.4 per cent. A careful study of the statistics of Schultz gives us 23 per cent. of definite failures. The results of these two eminent observers are so much alike that we are safe in considering them entirely reliable. With such a percentage of failures the question of surgical interference becomes pertinent. When and how are we then to operate? The indications for surgical interference may be said to be: first, when skilled treatment has failed to relieve; second, for large, intermittent, frequent hemorrhage; third, for very frequent or continuous slight hemorrhage; fourth, for perforation as soon after occurrence as possible; fifth, for suspicion of malignancy; and sixth, for the sequelæ, such as pyloric obstruction and gastric dilatation, food stagnation and hyperchlorhydria, pyloric spasm, adhesions with or without demonstrable tumor-like masses, perigastritis, and distortions the result of cicatrized ulcer.

The operative procedures to be considered are gastro-enterostomy with or without resection of the ulcer; resection, gastro-anastomosis, and pyloroplasty. At the April, 1906, meeting of the Deutsche Gesellschaft f. Chirurgie, the paper

of Kroenlein on the subject of the surgical treatment of gastric ulcer brought out a wide discussion on the lines of resection versus gastro-enterostomy. The leader of the resectionists, the veteran Rydygier, who had made the first resection of the stomach for ulcer twenty-five years ago and who was able to present the patient to the society, claimed for resection that it addressed itself to the disease direct and removed it, that it created conditions nearer the normal than any other operation, that it created better drainage, and that it compelled the admixture of the gastric contents with pancreatic fluid and biliary secretion at the normal point and removed the ulcer—in 5 per cent., at least, the cause of cancer of the stomach; that gastro-enterostomy did not address itself to the disease or its cause and produced only more favorable drainage conditions at best. Clinical experience and experiment are both opposed to this position.

Clinical experience and statistics show gastro-enterostomy to have a smaller mortality than resection; gastro-enterostomy in uncomplicated cases may be said to have a mortality of barely 3 per cent. The operative results in ulcer and hemorrhage are brilliant; operation promptly arrests hemorrhage and creates conditions most favorable to the healing of the ulcer or ulcers. In 50 per cent. gastric ulcers are multiple; the excision of one does not influence the fate of the others; unless old, with indurated edges and peritoneal thickening, or revealed by change in the color of the peritoneum over the site of the ulcer, they are difficult and often impossible to find. The situation of the ulcer might influence the choice of operation. An old, hard ulcer in or near the pylorus might be excised, and by a pyloroplasty the caliber of the pylorus be restored, stagnation of food prevented, and fair drainage secured; but other ulcers existing are not thereby relieved from the irritation due to stomach peristalsis and the passage of food over their surfaces. Even in that case—ulcers at the pylorus—gastro-enterostomy is preferable; the results are more permanent; pyloroplasty is unreliable.

Nor is it true that gastro-enterostomy does not meet the causal indication. The question of the cause of ulcer of the stomach has not yet been satisfactorily answered. Numerous experiments have been made (and our literature teems with them) which failed to supply the missing link. As bearing, however, directly on the question of the influence of gastro-enterostomy on stomach ulcer, the experiments of Fibich are distinctly interesting and valuable. He aimed to produce an ulcer in the animal resembling the round ulcer in man, with its chronicity and indisposition to heal. He had found that a simple trauma of the mucosa, as by the excision of a piece, always healed promptly in a few days and left a scarcely visible scar. After many trials the following method produced the desired result. Under anesthesia and with careful observance of the rules of asepsis a laparotomy was made, the stomach pulled forward, and near the pylorus two or three branches of the arteria coronaria dextra inferior ligated. A gastrostomy was made in the territory so deprived of arterial circulation, a piece of mucous membrane 1 cm. in diameter excised, and the margins cauterized with muriatic acid. The gastrostomy wound was then closed. Defects of the same size, by the excision of the mucosa alone, healed in a day or two; those by excision, cauterization, and ligation of the arteries, in three weeks. A gastro-enterostomy performed at once for such an ulcer resulted in healing in three days, as shown by postmortem examination. Ulcer without gastro-enterostomy, inspection by a second laparotomy after twelve days, showed no healing; a gastro-enterostomy was then made and five days later the ulcer was found to be healed. Enough of such experiments were made to bear out the statement that such ulcers, without gastro-enterostomy, did not heal; and promptly healed after that operation was done. To test the question whether healing after gastro-enterostomy was due solely to the draining away of the gastric juice, in two cases no gastro-enterostomy was made, but instead a gastrostomy with drain-

age, carrying the gastric juice away; healing did not take place. It was also found that after gastro-enterostomies there was generally a reflux of the alkaline juice from the small intestines, neutralizing rapidly the acid secretion of the stomach. It deserves further mention that, after reduction or disappearance of the acid in the stomach, the digestion of albuminoids was retarded or abolished, while fatty matters would digest in the stomach; the reverse held good in the upper intestine near the anastomosis. It would, therefore, appear that besides favorable drainage and the reduction of stomach peristalsis, a chemical change—the reduction of hyperchlorhydria, the cause or complication of gastric ulcer—was directly due to the operation.

The effect of surgical treatment on open ulcers is healing by cicatrization and restoration of the motor and secretory function of the stomach. Pronounced dilatation of the stomach gradually disappears, or grows less slowly when the dilatation has been of a high degree; the increased acidity lessens, and sometimes the acidity sinks below the normal at first, to reach the normal eventually.

With such demonstrations and clinical experience, gastro-enterostomy is the operation of choice for ulcers and hemorrhage, without excision of the ulcers and without hunting for bleeding points. This position has been taken strongly by Moynihan for some years and by some of our most expert surgeons. Kroenlein and Kocher hold similar views. We prefer the posterior gastro-enterostomy as done by Moynihan, the Mayos, and others. I made my first gastro-enterostomy with resection of the pylorus in 1891—first with the Senn plates; after that I tried the various mechanical aids, the rubber rings, the bone bobbin, the Murphy button, Laplace forceps, and others. In my last cases I did the posterior operation referred to above with perfect satisfaction, and in one case even under local anesthesia. Gastro-enterostomy will also prove the best operation in certain forms of hour-glass constriction of the stomach.

# ANEURYSMORRHAPHY FOR ABDOMINAL ANEURYSM.

## REPORT OF AN UNSUCCESSFUL CASE, WITH AUTOPSY.

By John C. Munro, M.D.,
*Boston.*

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The successful treatment of aneurysm of the abdominal aorta is most unsatisfactory, and it is in the hope that an occasional case may be relieved temporarily or permanently that the writer would offer the suggestion contained herein. Ever since Matas so brilliantly demonstrated the cure of peripheral aneurysm, by suture of the endothelial walls, it has seemed to the writer that if the same principle could be applied to abdominal aneurysms we might possibly save an occasional case, whose outlook under ligation, wiring, or other treatment is so doubtful.

Provided we could deal with a dilatation of the aorta, lateral or anterior, without involvement of too many important branches, it is within the range of possibility that we should compress the sac close to and parallel with the main trunk with rubber-covered clamps, open the sac and suture its lining surfaces together with successive rows of some absorbable material which would, for the time being at least, control further sac formation. We have a number of times ligated or packed the aorta in cases of aneurysm, but never with more than temporary relief. We have not tried wiring, though it has occasionally been successful in the hands of others. Ligation has been only temporarily successful. Most cases

of aneurysm come late, if at all, to the surgeon. Almost every one that the writer has seen has been treated as a spinal caries, a rheumatoid arthritis, or for some intra-abdominal lesion causing dyspepsia. Not until large, dissecting, pulsating tumors have formed have they come to the surgical clinic. If it were now permitted to examine these aneurysms early, before rupture, when there is a well-formed sac, and this sac were found to lie in such a position that it could be clamped and sutured, the writer believes that such a case could be successfully operated upon. This same method may be applicable to similar aneurysms elsewhere, where the surgeon would hesitate to sacrifice an important organ, like the kidney, or to open and suture the artery as laid down by Matas.

The case is reported merely as a suggestion, but the writer feels that the suturing can be depended upon to control leakage temporarily, and, on the basis of Matas' investigations, adhesion of a few hours will probably insure permanent union. In a second case, in which we opened the abdomen to try this method, the sac was so enormous and so tense that no clamp would compress the base without serious damage to the wall, and the operation was abandoned, the patient dying shortly afterwards from prolonged starvation and distress.

Case.—John M., aged thirty-nine years, had always been a hard worker in the fire department and as an expressman. He was not alcoholic and there was no history or evidence of syphilis. He was sent to me in May, 1906, by Dr. C. F. Painter, who two years before had treated him with a plaster jacket for symptoms of spinal disease. This treatment was continued for nearly a year with great relief, and the jacket was discontinued. Before this time he had been troubled with "stomach symptoms," and with pain extending into his groins and lumbar regions which had induced a surgeon to remove his appendix. No relief ensued until the jacket was applied, and although he was able to discard the latter

yet his dyspeptic symptoms did not entirely disappear. He described the pains as girdle-like, sharp and cutting, and starting in the dorsal region. The pain might last for a few minutes or an hour, and was worse in the morning. Rest and restricted diet modified the pain after the removal of the jacket. From the very first, three years before I saw him, the pain would at times shoot into the hips and along the outer side of the thighs to the knees, and he would camplain of soreness and lameness of the lower extremities. About eight months before I saw him he took a trip to Jamaica and derived great relief from his dyspepsia, but the pains in the lower limbs continued and recently they would shoot so severely into his hips that he would drop in the street. Within the last year he had become aware of a pulsating tumor in the abdomen which had lately caused him some discomfort. He was sent at once to the Carney Hospital for observation, and was placed on restricted diet, as recommended by Osler, and iodide of potassium.

Examination showed a visibly pulsating tumor in the epigastrium, dull on percussion, its lower edge coming three and a half fingers' breadth above the umbilicus. Pulsation was expansile and a loud bruit was plainly heard on auscultation. Otherwise, a tall, thin, muscular man, he appeared perfectly healthy. Under treatment the force of the pulsation diminished, and the tumor decreased about half an inch in diameter, by rough calculation. He felt so much better and so little could be offered him surgically that he went home, but returned five and a half weeks later suffering severely from pain, and so much weaker physically that he was willing to risk anything for relief. Operation under chloroform-ether anesthesia. On opening the abdomen in the epigastrium the aneurysm was found pushing forward above the stomach, with the pancreas resting on its lower portion. From the lower third the celiac axis passed downward, backward, and to the right, flattened against the aneurysm. This, together with the right phrenic artery, was ligated and

cut. The pancreas was then freed from the sac and the retroperitoneal tissues were pushed back from the front and sides of the tumor. At the upper pole the sac was as thin as paper and was torn, exposing a yellowish, soft, fibrinous tissue. At the time it seemed, and I hoped, that the upper portion was a false aneurysm and that I could find an opening in the true sac which could be dealt with by clamp and suture. I therefore explored with finger toward the celiac opening, but this was followed by a gush of blood which could be easily controlled by digital pressure. A rubber-covered stomach clamp was then passed parallel with the aorta and close to its anterior wall, so far as could be estimated, and the bulky sac clamped. It was necessary to place a small sharply-curved protected clamp at the upper part of the tumor to control the oozing from the rent just made. The tissues posterior to the forceps were then freed laterally, it being possible to work with comparative comfort. It then appeared that there was a mass of extravasation in the left loin with scarcely any pulsation. This was apparently a false aneurysm connected with that higher up. The only hope now lay in the possibility of controlling any opening in the main sac by our sutures, though it was felt that the chances were small. Ligation was out of the question because the upper limit of the tumor touched the diaphragm. Therefore, the sac controlled by the long clamp was opened lengthwise and some whitish clots turned out, leaving a smooth endothelial surface which was sutured face to face with three rows of continuous lockstitch chromic gut. On removing the clamps the sutured line was dry, but above, at the torn place where the wall was too thin and friable to allow suturing, there was bleeding which was controlled by re-applying the clamp. So far as could be told there was no leakage at the suture line, and had that been the only place for possible hemorrhage I felt that the abdomen could have been safely closed. Up to the last few moments the pulse had been slow and quiet, but with the loss of blood from the tear and the replacement of the

clamp, it increased rapidly in rate and lost volume. The clamp was left *in situ* and an iodoform wick packed about it; the abdominal wound being closed except for these. After ligation of the celiac axis there seemed to be no evidence of interference with the blood supply to the stomach. At the close of operation the veins became engorged, but this might easily have come from exposure and handling. The patient was put to bed in marked shock and was given salt solution and adrenalin. The Crile suit could not be applied on account of the projecting clamps. There was continuous oozing of blood and the patient died during the night.

Autopsy at the Carney Hospital by Dr. S. B. Wolbach, of the Pathological Department of Harvard Medical School. Body of a well-nourished, well-developed, and powerfully built white man, 182 cm. long. Rigor mortis complete. Postmortem lividity marked. Pupils equal, 0.6 mm. No edema.

*Abdominal Cavity.* The abdominal fat is 2 cm. deep. The edges of the opening in the gastrohepatic omentum are tied off with catgut sutures. The stomach is displaced slightly downward; the clamp and wicks rest upon the anterior surface and cross the lesser curvature about 4 cm. from the pylorus. The liver is slightly displaced upward and to the right. The jaws of the clamp enclose a mass of tissue 8 cm. long and, at the top where it mushrooms over, 1 cm. wide. Along the top are several stumps of tissue ligated with coarse catgut, besides a line of lockstitch, continuous catgut sutures. The whole surmounts a tumor mass lying upon the vertebræ and bordered on each side by crura of the diaphragm. The right crus is pulled forward by the tissue included in the clamp. There are 400 to 500 c.c. of liquid blood in the flanks. The great omentum below the operation wound is colored dark red with extravasated blood. The appendix is missing, the cecum is adherent anteriorly to the peritoneum, otherwise there is no indication of an operation. The site of the appendix is perhaps indicated by a few folds running to the

mesentery of the ileum from the cecum. The mesenteric nodes are not remarkable.

*Pericardial Cavity.* Normal.

*Pleural Cavities.* The left apex is adherent posteriorly by a few fibrous adhesions. The whole of the right upper lobe is adherent at the apex and posterior border by firm, fibrous adhesions.

*Heart.* Slightly large. Estimated weight, 350 grams. Myocardium is firm, uniformly brownish-red color. Valves and endocardium are normal. Coronary arteries show an occasional soft yellowish patch. The beginning of the aorta is thickly studded with small, elevated, white and yellowish areas; no calcification.

*Measurements.* Pulmonic valve, 8.0 cm.; tricuspid valve, 14.0 cm.; aortic valve, 9.0 cm.; mitral valve, 11.0 cm.; thickness, left ventricle wall, 2.0 cm.; thickness, right ventricle wall, 0.5 cm.

*Lungs.* The left apex is puckered posteriorly and the pleura slightly thickened, otherwise negative except for the postmortem settling of blood. The whole upper lobe of the right lung has scattered throughout caseous foci 2 to 6 mm. in diameter, separated by normal lung tissue. At the apex is a larger caseous focus 2 cm. in diameter just beneath the pleura. The remainder of the lung is negative except for postmortem settling.

*Spleen.* Elastic, firm, capsule slightly wrinkled. Estimated weight, 250 grams. On section the pulp is very dark red and firm enough to give a smooth surface. The Malpighian bodies are just visible as gray points. Trabeculæ not visible.

*Liver.* Estimated weight, 1600 grams; normal in size and consistency. Color is pale reddish-brown with delicate paler grayish mottling indicating lobular outlines. Gall-bladder and ducts are normal.

*Gastro-intestinal Tract.* Normal.

*Kidneys.* Normal in size. Estimated weight 350 grams.

Cortex averages 0.6 to 0.7 cm.; is pale. Glomeruli easily visible as glistening points. Pyramids pale red. Pelvic fat not increased. No cysts. The capsule strips with considerable difficulty, in places removing granules of kidney tissue.

*Adrenals.* Normal.

*Bladder and Ureters.* Normal.

*Prostate and Genitalia.* Normal.

*Thoracic Duct.* Patent, normal.

*Aorta.* The aorta is continuous above and below with a tumor mass over the lower dorsal and upper two lumbar vertebræ, and included between the crura of the diaphragm. The aorta was removed by stripping from the vertebræ, as far as possible, from above and below, until points were reached where it became firmly adherent to the spine (the upper level of the twelfth dorsal and the middle of the body of the third lumbar). These vertebræ were sawn about two-thirds through and the intervening vertebræ split off in the frontal plane. The aorta was then opened from behind, by sawing through the vertebræ and continuing the incision upward and downward. Over the twelfth dorsal, first and second lumbar vertebræ, is an aneurysm sac, 14 cm. long and 13 cm. inside, greatest circumference. The first and second lumbar vertebræ are excavated for a depth of 2 to 3 cm. The articular fibrocartilage is only slightly eroded anteriorly. The cavities in these two vertebræ are filled with firm grayish-red clot. The general shape of the sac is fusiform, and at the level of the first lumbar vertebra there projects, anteriorly, a second sac, 3 cm. deep and 4 cm. in diameter. The anterior surface of this sac, viewed from the inside, has a vertical slit 4 cm. long, the edges of which are enclosed in the pedicle clamp. The aneurysm contains but a small amount of clot, most of which lies against the vertebra where the eroded bone forms the posterior wall. The small anterior sac contains fresh dark-red clot. The aorta above and below the aneurysm, as well as the aneurysm, is thickly covered with wrinkled elevated white and yellow patches, thicker above the aneurysm

than below; there is no calcification. The common iliac arteries are very slightly sclerosed—the left one has one small patch only, the right has several large patches. The inferior mesenteric and renal arteries are patent. At the orifice of the left renal arteries is a conical bulging of aneurysm wall, about 1 cm. in diameter and depth. The celiac axis and phrenic arteries are included in the tissue between the clamp. The superior mesenteric artery is free but flattened by obliquity of its origin and is filled with grayish clot. The origin of the phrenic arteries, celiac axis, and superior mesenteric arteries is covered in the aneurysm by firm, friable, grayish clot.

*Anatomical Diagnosis:* Aortic aneurysm (abdominal); arteriosclerosis; pulmonary tuberculosis; absence of vermiform appendix; old appendix operation cicatrix; operation wounds (fresh); chronic fibrous pleuritis.

# NECROSIS OF THE SKULL; AN UNIQUE CASE.

By J. C. Wysor, M.D.,
*Clifton Forge, Va.*

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I wish to report a very remarkable and unique case of necrosis of the skull.

John H., colored, aged sixteen years, was admitted to the C. and O. Hospital, Clifton Forge, Virginia, on March 20, 1905. He complained of headache and had some elevation of temperature, and it was thought that perhaps he would develop a case of typhoid fever. A day or two later he called attention to a sore on his head in the occipital region near the suture between the bone and the right parietal. On examination there was found a collection of pus beneath the scalp. An incision was made to evacuate this pus and necrosed bone was found in the region of the original sore. A cresent-shaped incision was then made, and several square inches of both tables of the skull were removed, the skull being very thin, and the necrosis involved the entire thickness. The flap was then stitched back, leaving openings for free drainage. It was soon discovered that the necrotic process was still going on, and a crucial incision was made over the vertex and a much larger portion of bone was removed than at the first operation.

There was considerable hemorrhage from the longitudinal sinus. Openings were again left for drainage, and the flaps were stitched over the dura. The necrotic process still continued, and it was apparent that any further surgery was useless. Dressings were changed as often as required,

irrigating through the various openings in the scalp. He was given constitutional treatment consisting of various tonics and iodide of potash. There was no history or evidence of syphilis and he had received no injury. There was a steady, though very slow, march from bad to worse until his death, which occurred on July 19, 1906, sixteen months lacking one day from the time he entered the hospital. I have preserved all that remained of his cranium. He was able to walk about up to a few hours before his death. Owing to the fact that the muscles on the back and right side of his neck had lost their point of attachment, he could not raise his head from the pllow when getting up from the recumbent posture, but had to lift it up with his hands until he was erect.

The case is unique in the fact that all the bones of the cranium were involved except the left temporal. The right parietal, right temporal and occipital, except the basilar portion, are entirely gone, as well as nearly all of the right half of the frontal. A large portion of the left parietal and left half of the frontal are also destroyed. The sphenoid and ethmoid are involved. The articular condyle of the lower jaw on the right side was necrotic.

# CLINICAL DEMONSTRATION AT THE JOHNS HOPKINS HOSPITAL.

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## DERMOGRAPHIC TRACINGS FOR CLINICAL DEMONSTRATIONS.

By Howard A. Kelly, M.D.,
*Baltimore, Md.*

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Before proceeding to operate upon this patient, who has a large fibroid tumor, I wish to call your attention to something connected with the clinical registration of abdominal conditions.

We all have cases under observation where we should like to be able to decide, from time to time, whether or not a tumor is growing, or a mass has changed its position materially, or an exudate is being absorbed, but are met with the difficulty of preserving an accurate record of the condition. Memory is entirely untrustworthy. Now, for about a year I have been making use of a record made on a transparent cloth. I first make a careful outline in aniline, after a plan I devised while a hospital resident, and published in the *Philadelphia Medical News*, 1883, vol. xliii, p. 417. Following this method I draw the outlines of the tumor on the skin, getting as accurate a picture as I can of its periphery; then I take this nainsook or suisse, which is a sort of stiff transparent gauze, lay it on the abdomen, and mark out the ribs, the umbilicus, and the anterior superior spines of the ilia, after which I copy on it the outlines of the tumor. I always write

the name of the patient on the material, with the case-book reference, and then file it away. Then, when the patient returns, I make another entirely independent measurement and superimpose it on the old, so as to determine whether any material change has taken place in the size of the tumor or not.

This method is also valuable for making records of a variety of medical and surgical cases, as, for instance, enlargements of the stomach, misplaced kidneys, etc. These transferred dermographic tracings appeal to the eye in clinical demonstrations, and tell the story of the progress of cases from week to week and from month to month. They would be of value in thoracic conditions as well.

# DEMONSTRATION OF LAVAGE OF THE KIDNEY PELVIS.

By Guy L. Hunner, M.D.,
*Baltimore.*

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I wish to show you a case simply to demonstrate our methods of cystoscopy. While there have been attempts made to have a female cystoscope that will shed light within the bladder, and some of them with considerable degree of success, still we find it is more satisfactory to stick to the old method of using head mirror and reflected light. The greatest point in favor of this method is that you can always get light, whereas, with the other and more complex instruments the electricity gives out, or part of the mechanism gives out, at the critical time. Sunlight is the best you can have. So if possible, when examining a patient in her home, get her in front of a window where there is sunlight. A good diffuse daylight will suffice; but if sunlight is not available I prefer an ordinary lamp or tallow candle used in a dark room. For our work in the clinic or in our private offices the most satisfactory light is the electric light.

From the diffuse light in this operating-room the head mirror will collect enough rays to make a light to work by, but it is not as satisfactory as sunlight or an artificial light.

Men ask me questions about the difficulties of this work and why they fail. I always tell them that they do not get the patient in the correct position. The patient should be placed in the knee-chest, and not a knee-hand or knee-elbow, position. She may have a small pillow (as you see here)

under the chest. She should be far enough forward so that the entire leg from the knee to the ankle rests on the table. There is a little uncertain equilibrium where the patient's knees are close together, so they should be moderately separated. The thighs should slant slightly forward from hip to knee, and the back should have a downward bow—in other words, the patient should relax.

The nurse should be instructed never to leave a tight band of any sort, such as corset, waist-band, or abdominal binder, about the chest or abdomen. Of course, what we aim at in this position is to empty the pelvis, not only of the intestines, but of all pelvic organs; so that the pelvis becomes a potential vacuum to be filled with air when one of its hollow viscera is opened. We always open the vagina first, so that the vesico-vaginal wall balloons downward and carries the ureteral orifices into a position to be found easily when the bladder is opened. It is the non-observance of these little points which, I am sure, makes cystoscopy hard for the average beginner.

I could put this patient in such a position in a moment which would make it difficult for me to find the ureters. This is not really a good case on which to demonstrate cystoscopy and ureteral catheterization. She has had an old Neisser infection, with cystitis, which has left her with strictures of the ureteral orifices. As I bring the pressure of the cystoscope to bear it will enter the bladder with some difficulty. You will see this speculum jump as it passes the internal urethral orifice. In other words, the Neisser infection has left her with a stricture of the urethra. She has not only a stricture of the urethra, but a stricture of both ureteral orifices. This has not amounted to a pathological condition; it has not caused a damming back in the ureters; and there is nothing in the urine to indicate an inflammatory condition at the present time. For that reason I have not catheterized the kidneys before. The urine is perfectly normal.

I introduce the speculum about 3 cm., directly toward the lumen of the bladder, and as I withdraw the obturator

the bladder fills with air. Veering the speculum off to one side about 15° to 30°, all I see is the smooth bladder. This is a generally contracted bladder, and in going in 3 cm. the instrument goes beyond the ureteral orifice.

I do not see anything but the smooth posterior wall of the bladder. I must back out and watch for the ureteral orifices as I do so, expecting them to drop over the end of the speculum. I back out a centimeter, and there you see (indicating) is the ureteral orifice. You can see urine flowing in jets through the speculum now, coming from the minute pin-point orifice itself. The orifices are normally elongate slits in the mucosa; but in this case, as a result of a former cystitis, they are surrounded by scar tissue and have become minute, round, almost pin-point, openings.

Some one has asked about the wax-tip. The wax we use for tipping the renal catheter is composed of equal parts of sweet oil and white wax, melted and kept in this small test-tube. If I do not wish to use a catheter I can collect all the urine I want in this way; I can use an ordinary speculum, or, preferably, one of Dr. Kelly's oblique-end specula, find the ureter, and catch all the urine that comes from that side. In catheterizing the kidney you can grasp the outer end of the catheter with non-sterile hands, or, if you have plenty of assistants, one of them draws a three-finger sterile rubber glove over your thumb and first two fingers, and you may then grasp the catheter at any point and have a stronger hold. The assistant has made the end of the wax-tip just large enough to interfere with my getting into this very small ureteral orifice. We will use a smaller catheter with a wax-tip. The wax-tip ought to be passed carefully so as not to get a rub from the speculum. I find it is being obstructed at the orifice. It now slides over the ureteral orifice and I have it engaged. I had to make two or three attempts to engage the little round tip.

To wash out the kidney pelvis we ordinarily use small catheters, about this size (1.75 mm.) in diameter. This cath-

eter I hold in my hand is rather large for such a small ureteral orifice. We like to use a small catheter and gravity apparatus and let the fluid come back around the catheter. If you have a large catheter as compared to the size of the ureteral orifice, you can inject fluid into the catheter, take the injection apparatus away, and let the fluid come back, and then inject again. As soon as the catheters are introduced, the patient is allowed to lie on her side in a comfortable position while the washing is going on.

I see comparatively few cases of pyelitis that need irrigation; and as for treating chronic Bright's disease with pelvic lavage, I do not see the rationale of it. If we were looking for stone we would remove this wax-tipped bougie with care, and with a magnifying glass we can see if the tip has given the minutest impress of a stone; otherwise, we would see a perfectly smooth shiny surface.

I trust this demonstration may have impressed upon you the importance of having your patient in a correct position, if you would do cystoscopy by these methods.

# TREATMENT OF INFECTIONS BY VACCINES.

By R. I. Cole, M.D.,
*Baltimore.*

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The Secretary, who has honored me by asking me to speak before you this morning, requested that I should tell you of some of the work which we have been doing in the treatment of infectious diseases by vaccines. While our work has been mainly confined to the treatment of cases in the medical service, some of them have belonged in the "Grenzgebiete," and in these you may be interested.

There can be no question that at present the medical treatment of infections is unsatisfactory. The surgical treatment gives better results, but in many cases the surgeon cannot tell why or how the good result is obtained. Above all, it is the natural resisting force of the body upon which we have to depend. The surgeon recognizes this in the treatment of localized tuberculosis, and in fact of all localized infections, his object being not to eradicate the infection, but by simple incision so to change the environment that the natural resisting forces of the body become effective. It is, therefore, of as much importance to the surgeon as to the physician to learn how these resisting forces may be increased.

Resistance and immunity are closely related if not identical terms. We cannot go into a discussion of the historical development of our ideas in regard to immunity, but may say that up to a very short time ago there were considered to be but two main forms of immunity: first humoral, according to which the fluids of the body are thought to possess

properties which destroy the invading infectious organism or neutralize its products, and second, the cellular, according to which the main resistance is afforded by the cells, especially the leukocytes. It remained, however, for Wright, of England, to discover that while leukocytes are able to take up invading microörganisms and digest them, the presence of serum is necessary for this reaction; that leukocytes washed in salt solution no longer possess this property. Wright gave the name of "opsonins" to these substances in the serum which cause this action, and, furthermore, developed a method by which the amount of these opsonins in the blood can be determined with some accuracy. Based on these discoveries, Wright has developed a theory of immunity and resistance to disease. He has sought to increase the amount of immune substances in the blood by the inoculation of a corresponding vaccine, that is, an emulsion in salt solution of the dead bacteria of the same kind as those inducing the infection. He has applied this principle to the treatment of a considerable number of infectious diseases, controlling the administration of vaccines by the determination of the amount of opsonin in the patient's blood. He thinks that following the injection of vaccine, the amount of opsonin at first falls, and then rises to a much higher level. The first fall he calls "the negative phase" and the second rise a "positive phase." He thinks that the administration of vaccine during a negative phase may give rise to still greater diminution of the opsonin in the blood—what he terms a "summation of negative phases," and so do harm. He therefore considers it of great importance in the administration of these vaccines that frequent observations of the opsonic power of the blood be made.

We cannot here enter fully into a description of the method used in estimating the opsonic power, but will say briefly that equal quantities of the bacterial emulsion, an emulsion of leukocytes, and lastly, of the serum to be tested, are taken and mixed together and drawn into a small capillary pipette.

The end of this is then sealed. The pipette is now placed in the incubator at 37° for fifteen minutes, and then preparations are made from the mixture in much the same way that preparations are made for differential counting of leukocytes. At the same time that this preparation is made, a similar mixture is prepared, but instead of using the serum of the patient, normal serum is employed. Smears are also made from this and the whole technique is exactly that employed in using the test serum. Now under the microscope the number of bacteria contained in 50 polymorphonuclear leukocytes is counted and the average number per leukocyte is determined. This is known as the phagocytic index. The phagocytic index of the patient's serum divided by the phagocytic index of the normal serum gives what is spoken of as the opsonic index.

It should be stated that in our opinion these determinations are extremely difficult, and with the very best technique the margin of error is considerable. Whether this margin of error is sufficient to invalidate their use for the control of the administration of vaccines is still uncertain. It must be remembered, however, that the question as to the value of the vaccine treatment of infectious diseases is an entirely distinct one from that of the importance of the control of the administration of such vaccines by means of the opsonic index. While so far we have made frequent estimations of the opsonic power in our patients, and to some extent employed these determinations in deciding as to dosage, etc., nevertheless at present we prefer to speak briefly of some of our clinical results, rather than to speak of the value or importance of the opsonic index in this work.

We have so far treated a variety of infections, but I think you will be most interested in hearing of the results in some cases of gonorrheal arthritis, especially as several of these patients are here this morning. We have now treated by this method fifteen cases and they have all done well. In no case has ankylosis resulted. The cases were both acute

and chronic. The vaccine consisted of emulsions of gonococci in salt solution, killed by heating to 65° C. for one hour. The dosage employed varied. The first dose was usually 300,000,000 gonococci; later the doses were frequently increased. No ill effects were ever noticed following the administration of these amounts of vaccine. The intervals between the vaccinations averaged about eight or nine days, and the number required varied in the individual cases, but none of our cases have had to have more than eight doses of the vaccine. Four of the cases have consented to appear before you this morning, and all of these are at present practically well. One of these patients, with an acute arthritis of the knee of eight weeks' duration, recovered completely after three vaccinations and returned to work eighteen days after we began the treatment.

A second case, with arthritis of the elbow of seven weeks' duration, has completely recovered in three weeks, so far as his symptoms are concerned. He also is back at work.

Another one of our cases, a woman, had to have her joint (knee) opened and irrigated. Cultures from the joint gave a pure growth of gonococci. Following this she did not markedly improve, however, and vaccination was commenced. The improvement became much more rapid and the movement in the joint is now good.

It is impossible here to speak of these cases in detail and I must refer you to the complete histories, which will be published in order that you may come to a decision as to the value of this method of treatment. In any method of treatment it is somewhat difficult to judge exactly of the importance of the method, but our impression is that in these cases the improvement has been more rapid and the results have been better than one usually sees in a corresponding group of cases treated by other methods. The results in the chronic cases were even more marked than in the acute ones. In one case, a man who had had pain and marked stiffness of the knee for several months can now, after about a month

and a half of treatment, bend the knee a little over a right angle, the pain is all gone, and he has resumed his usual occupation.

In these cases we have omitted as far as possible all other forms of treatment, as we have wished to test the value of the vaccines alone. There is no reason, however, why they should not be combined with whatever other form of treatment may be considered advisable. One must not expect miraculous cures from this form of treatment. The vaccines are apparently a help, and the results are sufficient to justify us in continuing their use, but we recognize that it will take many more cases before we can obtain statistics sufficient to give us an idea as to the exact value of this method.

[The histories of these cases, together with more complete discussion, are published in the *Johns Hopkins Hospital Bulletin*, June and July, 1907.]

# REMARKS ON ANEURYSM OF THE ABDOMINAL AORTA.

By William Osler, M.D.,
*Oxford, England.*

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It may strike you as audacious for a physician to come into a surgical amphitheatre to talk about the medical side of aneurysm in the presence of so many distinguished surgeons; nevertheless, I have been asked to do so, but have only a few minutes to speak to you on the question of aneurysm of the abdominal aorta.

Aneurysm has a decided surgical side, and probably there is no more calamitous situation for a surgeon to find himself in than to find he has accidentally opened an abdominal aneurysm. The probability is, if a number of you were to search your surgical records and those of others, you will find you have been placed in that unfortunate position, and possibly some of you bave been placed in that position and have not recognized it subsequently.

There are three conditions under which a surgeon may operate upon an abdominal aneurysm and not know of it, and the first is, the rupture of a small abdominal aneurysm, with the passage of the blood into the retroperitoneum, may be mistaken for an acute appendicitis. Of course, nowadays, a pain in the abdomen, with a tumor situated in the right iliac fossa, means appendicitis and operation. I have known of two cases within the past three years in which the history was that of an acute abdominal pain, with the development of a tumor in the right iliac fossa, and operation for a suspected appendiceal abscess, but instead aneurysm was found.

I saw one such case only six or eight months ago. I likewise saw specimens from two cases in succession. Both patients were operated on, so that it is really not an uncommon thing.

A second condition is the presence of an abdominal tumor of large size, not pulsating to the ordinary somatic eye, but pulsating if you have a clinical and well-trained eye. The probability is that very few would suspect such a big tumor as that, occupying the whole flank and projecting to the back and in front, not pulsating at all, was an aneurysm of the abdominal aorta. But it was an aneurysm which ruptured into the retroperitoneum and formed a huge abdominal tumor. It is only occasionally that these tumors do not pulsate. Usually, with good eyes, if you watch closely in a good light, you can see a diffuse pulsation, but that is not always done. The pulsation may not be perceptible to the eye.

A third condition about which surgeons may be mistaken is preternatural pulsation of the abdominal aorta, pointed out in 1809. This condition is met with in young, nervous, and neurasthenic persons, and a point I would call attention to is that in a few of these instances in which pulsation is acquired, throbbing of the greatest intensity may be associated with recurrent attacks of hemorrhage from the stomach, very much the same as the post-operative cases of hemorrhage with which you are all well acquainted. I know of no single instance in which an operation has been done under such circumstances, but I have seen an instance in which a physician had suggested the possibility of ulcer or cancer of the stomach and the advisability of seeing the surgeon, but it was a neurotic patient, and having read of McKnight's description of identical cases in 1760, and having seen several other cases mentioned in the literature, I advised against operation. It was one of the cases of preternatural throbbing of the abdominal aorta with hemorrhage.

The points in the examination of these cases are well-known to you. In the first place, when a patient comes before you,

pulsation is usually obvious. Of the four varieties of pulsations which you meet with in the abdomen, by far the most characteristic really is that of an abdominal aneurysm. Pulsation of the heart itself, pulsation of the liver, pulsation of the abdominal aorta, and pulsation of the aneurysm, are the four, and by far the most striking is the pulsation of the tumor itself—a bloody tumor formed by rupture of the arterial coat. Pulsation may be communicated to a tumor of the stomach, to the pancreas, to the left lobe of the liver, or to any mesenteric or omental tumor over the abdominal aorta, and the pulsation of the tumor to the vessel itself. The difference is definite and well-marked if you can have the abdominal walls sufficiently relaxed to feel deeply the character of the pulsation, which has the same quality, the same intensity, and, of course, is of the same nature as the pulsation of the apex beat and of the heart itself. It has the same vigor under the finger, and there is the same inability to make an impression on it. Pressure intensifies the pulsation rather than diminishes it. If, as in this case, you can get the fingers on both sides of the tumor, then you can feel a distinct, elastic, expansile pulsation which is very, very characteristic. One thing you should bear in mind: There is no intensity of pulsation which may not be mimicked by throbbing of the abdominal aorta; a pulsation, for instance, greatly in excess of what you see in this case; a pulsation that jars the whole patient; a pulsation that jars the bed or jars you standing at the foot of the bed. We have throbbing of the abdominal aorta in certain cases of neurasthenia, or occasionally in cases of exophthalmic goitre. Remember, pulsation of the abdominal aorta in an abdominal aneurysm may be altogether posterior, and not, as in this case, anterior; and in a certain number of cases the tumor may project in the abdominal region just to the right or to the left of the spine, and these are the cases in which an examination anteriorly may reveal nothing, and if the patient is not examined carefully from behind a diagnosis may not be made.

A second important point is, the thrill felt on palpation may or may not be present in an aneurysm of the abdominal aorta. It may be present with a throbbing of the aorta non-aneurysmal. It is a variable feature. The bruit is also a very variable feature. It may be present and be intense, there may be a double murmur, or there may be no murmur whatever.

This case illustrates very well, indeed, the history of the majority of all cases of abdominal aneurysm. This man is thirty-six years old. He came to the hospital on the sixth day of December, 1906, complaining of pain in the upper abdomen, sides, and back. He had lues ten years ago. He informs us that he has been a hard worker all his life. He is a heavy drinker. He is a teamster by occupation. The patient while lifting a heavy weight had a sensation that something gave way in his abdomen. For a while after that there was pain, but this cleared up, and he had no symptoms until July, 1906, when he had a sudden pain in his upper abdomen. That has become worse, and has been with him up to five weeks ago, when the pain grew much worse. On the day of admission the pain was so intense that he had to drop his work and come to the hospital. He said he has lost fifty pounds.

Such is the usual history of these cases—syphilis, sudden strain, and a rupture, the syphilis producing what it usually does, a mesio-arteritis, often petty in character, sometimes localized in some region of the aorta, and not involving the intima. The intima may be smooth, but the media involved, and during a sudden strain a little slit-like rupture of the intima occurs, with the gradual production of aneurysm.

The usual etiological factors are very well shown in this case—Venus, Bacchus, and hard work, and the most important of these is probably Venus. In fact, without syphilis, in a man at this age of life, there would be no aneurysm in the great majority of cases, so enormous is the percentage.

As to the medical treatment of these cases, it consists of

absolute rest, low diet, and the giving of remedies like the iodides and remedies which aid the coagulation of the clot. Aneurysm is cured in some cases under those circumstances. There are a few instances in which the aneurysm is sacculated, with a small orifice, and in which healing has occurred spontaneously. In fact, the majority of cases of healed aneurysm of the abdominal aorta have been found accidentally. At the Philadelphia Hospital we had one instance of a woman, aged eighty-three years, who had three healed aneurysms of the abdominal aorta. This is one of the conditions in which surgeons have not been much more successful in treating than physicians. However, there is more hope really in surgical treatment than there is in medical.

We have had in this Hospital (The Johns Hopkins) a rather interesting experience with aneurysms of the abdominal aorta. Of this rare condition only about one case a year has occurred here. For instance, we had sixteen cases of aneurysm of the abdominal aorta in sixteen years, and of this number nine were operated on. In seven the sac was wired, and an electric current passed through it. In one case there was rupture into the pleura following. That illustrates a point, namely, you cannot tell when you have opened the abdomen whether or not that sac reaches up into the pleura on one side. And that is what happened in this instance I refer to. It was Case 5. There was a very prominent projection into the left pleura of which we knew nothing, and of which the surgeon was not aware, and following the operation rupture took place into the pleura. Case 8 died of hemorrhage into the peritoneum. Case 10 did very well; was discharged at the end of four weeks very much improved. There was marked diminution in the size of the sac. We have had no subsequent history of this case. In Case 11 there was rupture following the operation. Case 12 showed great improvement after operation and was discharged at the end of a month. We have no subsequent history of that case. Case 13 was a very interesting one. The patient had an agonizing pain and

a great deal of distress. The aneurysm was quite large. For three years following the operation the patient had good health; then rupture took place; so that altogether the record is not very satisfactory from surgical treatment. It depends, of course, on the character of the aneurysm and its situation. In the patient before us the sac is large. It has grown rapidly. There is great tenderness, as if the peritoneum was beginning to be involved. There is immobility, so that, in all probability, there is not much actual sacculation. It is not one of those pedunculated favorable tumors, and the question arises, what ought to be done in this case, whether the sac should be wired, or whether the case would not be a suitable one for attacking the thoracic aorta above and diminishing it by the method which Dr. Halsted has recently suggested. If we could see these cases early and make an early diagnosis, which can be done in quite small sacculated tumors by means of the *x*-ray with the greatest ease, either wiring or gradual compression of the aorta would be by far the most favorable method of treatment. There has been one completely successful case reported by digital compression—the case of Murray—and there has been one case treated successfully by ligation of the aorta. I do not know whether all other cases of ligation of the aorta have been fatal or not. If these cases are seen early, with the newer methods of operation, there is no reason why in a certain number of them operation should not be attended with success. In any case it is a desperate disease, and even though the remedies suggested are desperate, they are the best at our disposal, and success should attend our efforts in a limited number of cases.

# THE SUTURE OF BLOODVESSELS.

By Stephen H. Watts, M.D.,
*Baltimore.*

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The lateral suture of veins was carried out for the first time successfully by Schede, in 1882. This method was later used in numerous cases and has now come into such general use that most surgeons of experience have had occasion to use it.

To Jassinowsky, who published the results of his experiments in 1889 in his *Inaugural Dissertation*, belongs the credit of having proved conclusively that arterial wounds can be sutured with preservation of the lumen of the vessel. Of 26 cases in which he sutured longitudinal and partial transverse wounds of arteries of animals, all were successful except 4. As suture materials he used fine curved needles and fine silk, the sutures including only the media and adventitia and avoiding the intima. As the result of his experiments Jassinowsky reached the following conclusions: (1) The arterial suture heals by first intention; (2) bleeding after operation can be surely and completely avoided; (3) secondary hemorrhage and thrombosis are not to be feared; (4) suture should be done in all recent, clean, longitudinal and oblique wounds of large vessels and in transverse wounds not exceeding one-half the circumference of the vessels; (5) the strictest asepsis is necessary; (6) the suture is easily done.

Jassinowsky's results have been confirmed by Burci, Murphy, Dörfler, and others. Numerous instances of suc-

cessful suture of accidental wounds in the arteries of man have demonstrated the practical value of this procedure. In 1902 Schmitz was able to collect 21 cases of lateral suture of arteries in man.

A good deal of experimental work has also been done on the circular suture of bloodvessels. As the result of these experiments various methods have been recommended for uniting divided vessels. In general these methods may be divided into two classes: (1) Those in which the simple suture is used; (2) those where mechanical aids are employed. Among those who have advocated the simple suture are Briau and Jaboulay, Murphy, Carrel, Jensen, and others. Some have used simple interrupted sutures and mattress sutures; others the continuous suture. Some include all the coats of the vessel in the suture, others avoid penetrating the intima. Carrel, who has obtained more brilliant results with bloodvessel suture than any other experimenter, in his original article advised the avoidance of the intima. In 1897 Murphy after numerous experiments on animals recommended uniting completely divided vessels by invaginating one end a short distance into the other and fixing them thus with sutures. On account of the additional support thus obtained he thought the method particularly applicable to arteries. In the majority of cases of suture of divided arteries in man, this method has been used.

The mechanical aids may be divided into the extravasal and endovasal. Among the former may be mentioned various clamps, decalcified bone and ivory rings, sheaths made of sections of other arteries or veins, aluminum rings, magnesium rings, etc. The most important of these are the magnesium rings or protheses of Payr, by means of which the ends of the vessel are so invaginated that intima is approximated to intima. Payr and Höpfner claim to have obtained good results with this method. The endovasal aids are of no importance clinically; the glass cylinder method of Abbe is, however, of historical interest.

Arteriovenous anastomoses have been done by San Martin, Carrel, Jaboulay, and others. The interesting results obtained by Carrel are doubtless familiar to all of you.

The following experiments were carried out with the strictest aseptic technique, for the most part according to the method suggested by Carrel. The animals were cleaned up with soap and water, alcohol, permanganate of potash, oxalic acid, and bichloride. After making the skin incision sterile towels were clamped to the edges of the incision.

The vessels were exposed and provisional hemostasis produced by small bull-dog clamps covered with rubber. I might say here, that in none of the specimens was there any evidence of injury to the vessel wall produced by these clamps, with the possible exception of one or two of the veins. The vessels were then divided and prepared for suture. Very fine straight needles and fine silk were the suture materials used. I will pass around some of the needles and thread. The thread is greased with or boiled in vaselin as suggested by Carrel.

The vessel ends were first united by three interrupted sutures equidistant from each other, the suture penetrating all the coats of the vessel. By traction upon these sutures the edges of the vessel wound were nicely approximated and a continuous suture easily applied. The leakage from the stitch holes was usually slight in amount and easily controlled by a little pressure. The tissues covering the vessel were sutured with fine silk and the skin with a subcuticular suture of the same material.

The carotid artery was thus sutured thirteen times. All were successful and in no case was there the slightest evidence of thrombus formation. The jugular vein was also sutured thirteen times, ten of these being successful. The femoral artery and vein were sutured once, thrombosis occurring from infection of the wound.

The carotid artery was sutured to the jugular vein four times, all being successful. Following the suture the veins

became dilated and pulsated strongly, a marked thrill could be felt in the vein and a loud murmur could be heard over it. Examination of the veins after one to three months showed a marked dilatation of the vessels, a thickening of the walls, and in some there were interesting plaques in the intima.

The proximal end of the divided femoral artery was sutured into distal end of the divided femoral vein four times. One was successful; in the others thrombosis occurred. In the successful case the leg became very much swollen after the operation, and in two days the leg was twice as large as the opposite one. The swelling gradually subsided. The femoral vein became much dilated and marked pulsation could be felt in the saphenous vein as low down as the foot. The specimen examined after three months showed marked dilatation of the vein and thickening of its wall. The distal portion of the femoral artery was small, atrophic, and did not seem to be performing the function of a vein.

In four animals a lateral anastomosis was made between the femoral artery and vein. In all cases the anastomosis was quite satisfactory. On turning on the blood current there was no leakage, the vein became considerably dilated, a thrill could be felt in the vein near the point of anastomasis, and a bruit could be heard at some distance from the animal. The red arterial stream could be seen through the thin-walled vein, rushing into the vein and turning upward with the venous current. In only one of the animals, however, did the anastomosis remain patent, a marked thrill and loud bruit being present five weeks after the operation, when the animal escaped and was thus lost. In the second the murmur and thrill persisted for four weeks and then disappeared; examination a few weeks later showed that the opening had healed; the vessels, however, were patent. In the third animal the thrill and murmur lasted only four days; wound infected; examination two months later showed thrombosis of the artery opposite to the anastomosis, the vein being patent. In the fourth the thrill and murmur persisted five or six days;

secondary hemorrhage occurred on the eighth day after operation.

Excision and replantation of a section of the femoral artery was tried once. Thrombosis occurred. Transplantation of a portion of a vein into an artery was tried twice. In one instance a portion of the external jugular vein was transplanted into the carotid with perfect success, as the specimen removed after three weeks will show. In the other case a portion of the external jugular was transplanted into the femoral artery. Thrombosis occurred in a few days.

In reviewing these experiments we see that, out of 31 experiments upon the vessels of the neck 28 were successful, while out of 12 experiments upon the femoral vessels only 2 were entirely successful. This discrepancy is not difficult to explain. The neck can be bandaged, the wounds thus being kept clean and dead space obliterated. In the groin, however, it is very difficult to apply a bandage which will remain in place, obliterate the large dead space which is so apt to be present, and prevent movements of the leg which interfere materially with a successful vessel suture.

These results show conclusively that completely divided vessels can be sutured with almost uniform success when the aseptic technique is good. The intima can be included in the suture with impunity, the application of the suture being thus greatly facilitated.

# REMARKS ON SOME FURTHER MODIFICATIONS IN THE GASSERIAN GANGLION OPERATION FOR TRIGEMINAL NEURALGIA (SENSORY ROOT EVULSION).

By Harvey Cushing, M.D.,
*Baltimore.*

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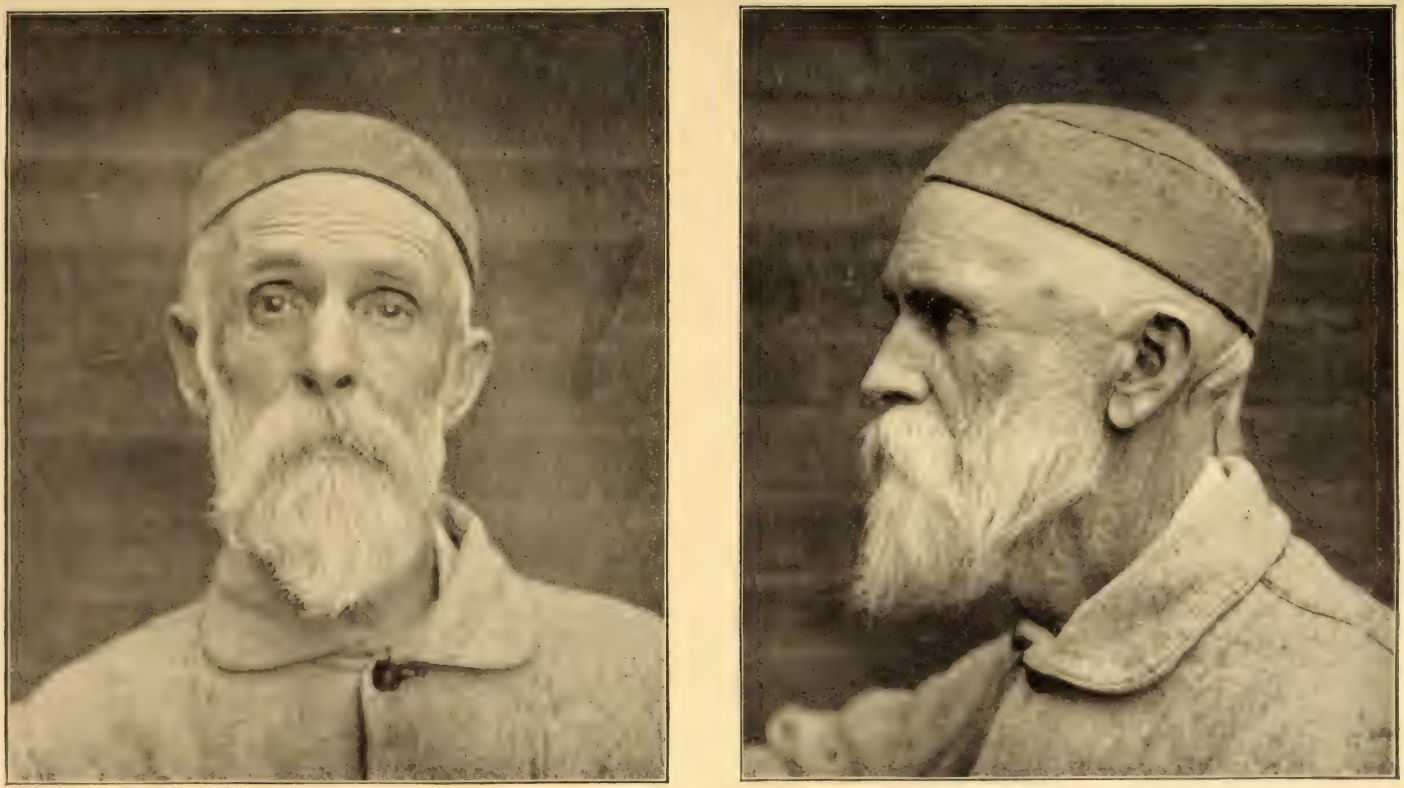
The request was made, that, if it were possible, I should have a patient here this morning for operation. If this was expected of me, I regret that I must disappoint you. In my last patient, the forty-ninth case in my ganglion series, a trigeminal neurectomy was done two days ago, and had she not been suffering greatly, I might have held her over for this occasion. However, even were it possible to demonstrate the steps of this operation before a large gathering, I have certain scruples against making a display of a procedure of this kind, for to do so is hardly fair either to patient or surgeon.

The operation is difficult, delicate, and, for its successful completion, demands the undistracted attention of the operator for a period which, in a difficult case, may be drawn out to fully two hours. The moments when it is possible to demonstrate to onlookers, one at a time, the stage of the deeper enucleation are moments which should be taken advantage of for further progress in the dissection. Surgeons who will attempt these procedures hurriedly, in the middle of a morning's work, or who will attempt to make a show of them, will certainly meet with disaster in a difficult case, and either leave the patient in a critical condition or the operation unfinished. Owing to the depth at which the



Case XLI.—Patient aged sixty-four years, whose neuralgia had been of eighteen years' standing. No preliminary peripheral operations attempted. Photographs taken six days after operation, showing practically "invisible scars" and symmetrical wrinkling of forehead.

structure lies, and to the small cranial opening through which it is approached, a continuous, clean, working view of the ganglion is possible for the operator alone, and even he is restricted to monocular vision. The assistant at the wound rarely has more than an occasional glimpse of the structure until the termination of the enucleation.

I have insisted, over and over again, upon the necessity of observing the strictest detail in these operations, for in this way alone may a series of completed operations without a fatality be carried through. In our last twenty-five cases there have been no deaths and no post-operative complications.

I will pass about among you a series of photographs of most of these patients, showing the situation of the operative wound and the areas of post-operative anesthesia. Attached to each card is a brief paragraph which is descriptive of the individual case. What I would have you note chiefly in these photographs is the situation of the superficial incision, which is so placed as to avoid the branch of the facial nerve which supplies the occipito-frontalis (pars frontalis). So far as I know, in all methods of approach other than that which we employ, this twig of the facial nerve is unavoidably injured, with consequent slight sagging of the brow. Many of these photographs were taken to show that the patients were able to symmetrically elevate their brows and wrinkle their foreheads.

We have two distinct problems before us in this operation, which are met by different surgeons in different ways: they concern (1) the method of approaching the ganglion and (2) the method of treating it when it has been brought more or less completely into view.

1. One should *approach the ganglion* in such a way as to encounter the least difficulty from hemorrhage; also in such a way as to avoid even trifling injury to any expressional structure, so that the post-operative deformity may be minimized. Both of these objects are best accomplished, I believe,

by the operative method which was employed in these patients, and unless tests were made for anesthesia, even an observant physician would be unable to tell that they had been subjected to this supposedly major procedure. It is lamentable that, after corresponding with others already operated upon, these patients, of their own accord and often contrary to their doctor's advice, must continue to seek the permanent relief which a ganglion operation alone affords, having been told that loss of the eye, facial paralysis, and worse are its usual consequences.

Practically the only modification that has been made in the approach, since my first description of "the operation under the meningeal artery,' lies in this changed position of the incision. The operation is always done without injury to the meningeal, which remains attached at its two anatomically fixed points, namely, in the parietal bone and at the foramen spinosum.

One thing further to which I would call your attention in the photographs is the practically invisible scar of this incision. It is almost impossible to tell in some of these patients, even a few days after the operation, where the incision has been made, and consequently it has failed to register on most of these negatives, though they are for the most part very clear. There is, furthermore, no observable deformity from muscular atrophy. This is due partly to the removal of the zygoma, and partly to the exceedingly careful and accurate closure in layers, at the end of the operation, of temporal muscle, of temporal fascia, of galea aponeurotica, and finally of the skin.

2. The *treatment of the ganglion* is the essential part of the operation. Those who have encountered difficulties in the free exposure of the structure have naturally come to advocate measures which must be regarded as incomplete and uncertain in their permanent effects. Among them are removal of the lower two-thirds alone of the ganglion; the interposition of rubber tissue between the divided mandibular and maxillary divisions; the attempts to obstruct the foramina

of these two nerves by plugging them with various substances; the injection of osmic acid, etc. Even should these measures seem to permanently cut off impulses from the cells of the second and third divisions, what may we say of the ophthalmic division which in all severe cases ultimately becomes involved, even if it has not been the primary seat of pain?

To insure permanence of relief we must be certain either of removing all the ganglionic cells, or else of so treating the sensory root, between ganglion and pons, that there is no possibility of reunion of the fibres.

There can be no doubt but that total extirpation assures a cure. It is the method followed by Horsley, Krause, and Lexer, who possibly, all told, have done, successfully, more of these operations than all other surgeons together. In my first 30 cases I attempted to remove the ganglion in toto and was successful in doing so in 26 of them. I lost two of these patients, one of them clearly from shock due to the prolonged manipulations of the nerve. Of the four patients with the necessarily incomplete extirpations (lower two-thirds only of the ganglion removed), two of them, after some years, have had a recurrence of pain in the remaining ophthalmic division.

At Spiller's suggestion, Frazier, using the method of approach which I had described, endeavored to simplify the treatment of the ganglion by simple section of the trigeminal root proximal to the ganglion cells. Dr. Frazier has succeeded in carrying out this measure in certain cases which he has recorded. Owing to the character of the dural attachments, however, I do not find it always possible, even after the ganglion is well exposed, to bring the sensory root into view, and it is my impression that, when one can do so, total enucleation is usually easy. Furthermore, we are not yet assured that there can be no reunion of function after simple division of a sensory nerve root, provided the severed ends are left sufficiently near for a neuro-tropic influence to act on the outgrowing axones from the cells of the root-ganglion.

Could we, in a large percentage of cases, easily expose the sensory root, and could we be assured that after its division no regeneration were possible, there is no doubt but that this measure would become the favorite one among those who have made a serious study of this operation; for it does avoid, as Frazier has said, the delays and difficulties from venous hemorrhage which are encountered during the process of freeing certain of the trigiminal divisions—the ophthalmic in particular.

*Root Evulsion without Removal of the Ganglion.* I think that I have been able to overcome the objections to the total enucleation procedure and at the same time to assure an impossibility of reunion, and thus a certainty of permanent relief, by a simple withdrawal in toto of the trigeminal root—a measure which is possible even in those cases in which the root cannot be fully exposed to view.

In one of the later cases of my earlier series—a case in which the operation had been delayed by considerable bleeding, brought about during the process of freeing the maxillary division—I succeeded in evulsing the trigeminal root, together with the lower part of the ganglion. On another occasion the sensory root came away from the pons in toto and broke off where the evulsing clamp had been applied to the ganglion, and as, under these circumstances, there was no reason for removing the sensory cells themselves, the ganglion was left. These cases finally led me to attempt the avulsion alone without disturbing the ganglion, and the method has proved so satisfactory that it has been followed in the patients whose photographs I have shown you.

Briefly, when once orientation has been assured by bringing into view the maxillary division, the dural envelope is split between this nerve and the mandibular division at the foramen ovale. From this time on attention is paid solely to the mandibular division, and in particular to its posterior margin. By working with the proper blunt dissectors between the meningeal artery and this margin of the nerve, one may

finally free the outer edge of the ganglion up to the smooth dural opening, through which, under the attachment of the falx, the sensory root passes from its free course in the posterior cranial fossa over the crest of the petrous bone and into the cavum Meckelii of the middle cranial fossa. A special form of spatula, which I shall show you, has been devised to hook under the upper edge of the dural ring, which transmits the sensory root. It serves, at the same time, to elevate the temporal lobe to the slight degree which is necessary in this procedure. A narrow, blunt dissector is then insinuated under the posterior margin of the ganglion, about at its junction with the root, and by gentle pressure upward and forward the entire root is avulsed from the pons and is curled forward into view into the middle fossa. The ganglion, only a small portion of which has at any time been exposed, is left in its bed, with a gap of 2 cm. or more between its cells and the pons, surely sufficient to prevent any possible physiological reunion of fibers in the future.

In none of these patients has the operation occasioned the temporary pressure palsy of the abducens which formerly was a not infrequent sequel of the total extirpation procedure.

# EXPERIMENTAL PARTIAL OCCLUSION OF ARTERIES.

By W. F. M. Sowers, M.D.,
*Baltimore.*

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I am sorry Dr. Halsted cannot be here to present this subject. The experiments in partial or progressive arterial occlusion in which I assisted were started about two years ago. They were made on dogs and in great part on the abdominal aorta. Partial occlusion of arteries was thought of by Luigi Porta about the middle of the last century, and gradual occlusion by means of instruments carrying a snare in some form was attempted by several surgeons in the last century. Usually the snare-ligature was placed about the vessel, its ends brought out of the wound, and through the open wound it was drawn tighter from day to day. This method has always been attended with infection and is quite inconceivable without it, hence our effort has been to devise a method in which, for one thing, infection should be avoided. Progressive occlusion of an artery seems to be a plausible idea because it allows the gradual establishment of a collateral circulation and may bring about total occlusion in such manner that there shall be no danger to life or limb from gangrene or from insufficient nourishment distal to the point at which the constriction is applied. It is interesting to note that Luigi Porta, whose book I pass around, in 1846 applied strips of diachylon plaster to the abdominal aorta of dogs in an attempt to partially occlude the vessel. At this early date operation was, of course, without antisepsis and Porta's

results were accordingly vitiated by the element of infection. Dr. Brewer's ingenious method of applying absorbable adhesive straps in order to close wounds of arteries must he mentioned.

Our efforts, then, were to partially occlude arteries by ligatures which could be tightened in two or possibly three acts, and which would not tend to cut through or injure the vessel wall or predispose to sudden hemorrhage, and which would not cause thrombosis and sudden complete occlusion. To accomplish this we thought it might be well to exercise an equable pressure all about the vessel wall and to use a broader ligature—one which would not injure the arterial wall—than an ordinary wire or thread. Once or twice we partially occluded the abdominal aorta with a heavy silver wire but soon abandoned this material as impracticable. Next we tried silver bands of varying degrees of thinness;[1] these were rolled at first digitally about the vessel. Soon we succeeded in having made a machine to accomplish the initial rolling and the placing of the band in position on the vessel. The final tightening of the bands is accomplished by rolling with the fingers. The bands are stiff and long enough to maintain the constriction. Later we found that aluminum bands rolled better and easier into the cylindrical form than silver, and for this reason most of the later operations have been done with aluminum. It has developed that a thin, rather narrow band (the width perhaps of one-third or one-half the lumen of the vessel), rolled with the fingers, can be applied with less trauma and greater evenness than a thicker, wider band which could be rolled only with the aid of forceps.

EXPERIMENTS. These bands in our aortic experiments (over ninety dogs have been operated upon) were applie just above the bifurcation of the abdominal aorta, above the renals, and between these two points. More recently Dr. Halsted has applied successfully a band which ultimately

[1] The sizes—sheet metal gauge—most commonly used are from 29 to 36.

brought about complete occlusion of the thoracic aorta. This operation was done May 22, 1906; the dog is still alive. The ligature was rolled quite tightly so that a well-marked thrill (only a very feeble pulse) was demonstrable below the band. Total occlusion took place spontaneously a few weeks later. The question of accurate measurement of the degree to which in a given case the lumen is reduced has been a difficult one; we have attempted to determine the blood pressure above and below the band by means of a small hypodermic needle introduced into the blood stream and connected with a manometer; but the needling of the vessel wall predisposes to thrombosis and hence is a dangerous procedure. We have had to rely chiefly on finger palpation in estimating the blood pressure. The bands have been rather loosely applied in some cases; in others, tightly; often they have been rolled until the blood stream was almost occluded, and occasionally the arterial lumen has been completely obliterated. Where there was complete occlusion of the aorta, and occasionally when it was not quite complete, there resulted paralyses of the hind legs. This paralysis commonly diminished greatly within from three to ten days in the dogs which recovered. Most of the smaller dogs with complete occlusion succumbed quickly.

Results. 1. Bands so applied as not to completely occlude the lumen may remain in place on the arterial wall in the case of the aorta, femorals, carotids, and renals for months without causing macroscopic injury to the walls of the artery. Usually there was very little or no reaction in tissues. The bands on the abdominal aorta when the dog is killed—say three months after the application of the band—can be seen shining through an almost normal peritoneum covering them.

2. Bands causing slight compression of the artery can be removed at any time, and probably without risk of subsequent dilatation of the arterial wall; or they may be tightened at a second operation and the lumen totally occluded. An ordi-

nary silk ligature has been used occasionally to complete the occlusion.

3. The intima inside the occluding band is drawn into folds, and when the dog is autopsied and the vessels opened longitudinally adhesions between the folds of the intima beneath the band have rarely been found. The vessel wall can be easily smoothed out and the full lumen re-established.

4. When the bands have been rolled tightly enough to completely interrupt the circulation, the vessel wall inside the band has been atrophied. Sometimes there has been complete absorption of the aortic wall. Hemorrhage in such cases has invariably been prevented by the formation of enveloping connective tissue.

5. Complete thrombosis of the vessel wall when a partially constricting band has been applied has not been observed in a single instance. In one or two specimens a very small caruncular nodule, representing probably an organized thrombus, has projected very slightly into the lumen.

6. In no case save one since the development of the technique has there been a death from hemorrhage following application of the band. One of the earliest dogs in this series—a puppy with a thin-walled aorta—after several attempts to apply the band, associated with a good deal of trauma to the vessel, died from hemorrhage due to a cutting through of the wall of the aorta by the edge of the band twenty-four hours after operation. The fact that this calamity has not repeated itself we attribute to the absence of trauma and to the care with which the band's edges have been smoothed with the file.

Demonstration. These specimens show the condition of the arterial wall after longitudinal division of the band and of the vessel. They represent (1) cases in which incompletely occluding bands were left in place for from two to six or more months, and (2) cases in which spontaneous and complete occlusion has ultimately taken place after the application of bands which almost occluded the artery. These specimens

show the ideal closure of an artery by the adhesion of the opposed surfaces. The folded arterial wall has, as you will note in two of the specimens, been converted into a solid fibrous cord.

Practical Applications of the Method. 1. In cases of aneurysm where ligation is attended with danger of gangrene.

2. In place of ligature of the common or internal carotid. The occlusion may be produced gradually; and, subsequently, should cerebral symptoms develop, the band may be removed.

In five human beings bands have been put on the common carotid on one side coincident with ligation of all or most of the branches of the external carotid on both sides, for starvation of inoperable malignant neoplasms involving the face and upper part of the neck and jaws. Dr. Halsted has also successfully applied a band to the innominate artery in the treatment of subclavian aneurysm.

3. Aneurysms of the aorta may, we think, be treated by making a partial or progressive occlusion of the aorta above the aneurysm.

4. The band is possibly safer than the ligature for calcareous arteries.

5. In certain cases of rupture of the diseased thoracic or abdominal aorta a metal band might be used to close the rent in the wall of the vessel.

[Exhibition of the instrument and demonstration of the method.]

# SUPRAPUBIC HYSTEROTOMY.

By W. W. Russell, M.D.,
*Baltimore.*

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In May, 1906, I published in the *Johns Hopkins Hospital Bulletin* a paper entitled "Suprapubic Hysterotomy as a Means of Diagnosis and Treatment of the Uterus." I cited 34 cases in which the operation had been applied. Since then other interesting cases have been met with in the course of our work.

In a very kind letter from Dr. William J. Mayo, received some time ago, he spoke rather enthusiastically of the usefulness of this procedure and regretted that I had not written a more elaborate paper. My excuse for this is that the procedure is as yet to a certain extent in an experimental stage, and I did not wish to appear too enthusiastic about advising its application. It is as follows:

A uterus which is suspiciously enlarged or which has given rise to intractable, unexplainable hemorrhages is exposed either in the course of an abdominal operation for other purposes or in the course of a purely exploratory operation where the abdomen has been opened for the particular purpose of doing a hysterotomy.

After the pelvis organs have been examined, the uterus is drawn out of the abdominal opening as far as the cervical portion and surrounded by gauze on all sides. It is then grasped below on both sides by an assistant who lifts the body well up while the operator boldly splits it in the median line on the anterior wall, half way down toward the neck,

as well as through the fundus to the posterior surface. The entire thickness of the uterine wall is divided for a distance of two or more centimeters down into the cavity. The mucosa can then be inspected at every point, the finger can be inserted and the whole cavity felt, and if there are any nodules in the wall they can be palpated.

If the mucosa is diseased, it can be scraped away with surprising ease and with the certainty of reaching all points efficiently, even in the cornua about the entrance of the tubes, a procedure which is almost impossible by the cervical route. This is of particular value, as in some of our cases delicate polypi were found which had not been reached by previous curettings. The bleeding is only moderate and it has not been necessary to put ligatures around a vessel.

The wound in the uterus is closed with interrupted catgut sutures passed from the peritoneal surface down to the mucous membrane. These are placed about ½ cm. apart, leaving the detail of closer approximation to fine catgut sutures. Catgut sutures may, if properly sterilized, be employed with safety in cases where there is not history of infection, otherwise fine silk is preferable.

Suprapubic hysterotomy is of conspicuous service in cases of persistent uterine hemorrhage which is not controlled by any form of treatment and in which bimanual palpation of the pelvic organs has given an absolutely negative diagnosis. In not a few instances the mucous membrane will show by microscopic examination a polypoid change, yet thorough and repeated curettings do not stop the irregular bleeding. This is explained, as already mentioned, by the fact that the growth is in the uterine cornua and also because none of the instruments devised will successfully reach these areas.

Small submucous and pediculated myoma which do not affect the size or contour of the uterus, but which may be the source of dangerous hemorrhage, are easily detected and removed by this method. Where the abdomen has been

opened for some other purpose and no explanation has been found for the uterine hemorrhage, I would then also advocate its application.

No deaths have occurred in the 35 cases thus far operated upon.

# OPERATION: CONSERVATIVE PERINEAL PROSTATECTOMY FOR CHRONIC PROSTATITIS.

By Hugh H. Young, M.D.,
*Baltimore.*

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The patient whom I have selected for operation before you is one of chronic prostatitis and not of prostatic hypertrophy. I have selected this case because I think the subject of the operative treatment of chronic prostatitis is one of more interest at present than that of prostatic hypertrophy about which so much has been written. In a recent study of 358 cases of chronic prostatitis[1] we found 34 cases in which definite obstruction to the free evacuation of urine was present in varying degree. In some cases there was only a slight bar, very little residual urine, the principal lesion being a contracture of the bladder produced by the muscular hypertrophy brought on by the increased difficulty of urination.

In 4 cases the residual urine varied from 65 to 150 c.c. In 1 case was 440 c.c. and in 2 cases complete retention of urine was present and the patients led catheter lives. Since the publication of this article 2 additional cases of complete retention of urine and catheter life of long standing due entirely to chronic prostatitis in young men have been encountered.

There have also been a number of cases in which no obstruction to urination was present, but the pathological process was so severe and so rebellious to the ordinary methods

[1] Studies in Urological Surgery, Johns Hopkins Hospital Reports, vols. xiii and xvi.

of treatment, that it was thought wise to make use of operative treatment for a more radical removal of the chronic inflammatory tissue. In 6 such cases the operation of conservative perineal prostatectomy has been performed and the results have been so gratifying that I am inclined to believe that time will show the great advantage of operative treatment for the more severe cases of chronic prostatitis, even though no obstruction to urination exists. For those cases associated with obstruction there can be no question as to the advisability of operative interference, as shown by the beautiful results which have been obtained in my cases.

Our patient today is a case of severe chronic prostatitis with median bar formation, considerable obstruction to urination which has led to hypertrophy and contracture of the bladder. He has also suffered from severe hemorrhages from the posterior urethra which have continued up to the present. The prostate is not hypertrophied and will, therefore, not be nearly so satisfactory for the ocular demonstration of the operation of "Conservative Perineal Prostatectomy" as would have been possible with a case of prostatic hypertrophy, and the markedly indurated, fibrous condition of the prostatic lobes will make the operation much more difficult than if we were dealing with large, globular, easily enucleable lobes. At the same time I hope to be able to show you by this case the advantages of the operation which I employ in even the most difficult cases.

The operative technique is shown in the diagrams which hang upon the wall; these are the same which have appeared in the TRANSACTIONS of this Association for 1905. The proper position of the patient is of great importance. As you see I make use of Dr. Halsted's perineal board, by means of which it is possible to greatly elevate the hips so that the perineum is almost parallel with the floor. After the inverted V skin incision has been made (Fig. 1), the space on each side of the central tendon is opened by blunt dissection with the handle of scalpel and finger until the triangular ligament

is reached. A bifid retractor which I have lately devised is now introduced and is of great assistance in dividing the central tendon and rectourethralis muscle to it without injuring the bulb. As you see now I have cut no important structures and with the exception of the two insignificant median line muscles the rest of the exposure has been made by blunt dissection. In this case there is more cicatricial tissue than usual around the membranous urethra, the exposure of which you see is somewhat difficult; but by proceeding carefully I hope to expose it without injury to the rectum which lies very closely at hand. As you see now I have pushed back the rectum for a short distance, the membranous urethra with the sound distending it is exposed to view, and I shall make a longitudinal incision into it (Fig. 2). You now see the sound shining through the urethrotomy wound and I shall proceed to pick up the edges of the mucous membrane with artery forceps. It is very important to catch the mucous membrane, as most of the difficulty experienced in introducing the tractor comes from invagination of the mucous membrane, due to the failure of the operator to secure well the whole urethral wall before attempting to introduce the tractor.

I have now secured both edges of the mucous membrane, and having withdrawn partially the sound which is in the urethra I shall pass another sound through the urethrotomy wound into the bladder. Having removed this I now pass the prostatic tractor closed (Fig. 3) into the bladder and immediately open it out (Fig. 4). Traction is now made upon it and you see how much closer the prostate is drawn down into the wound. As you see it is really quite close to the skin, but the rectum still covers most of its posterior surface and in freeing it very great care must be taken else we tear it. I am careful to work against the posterior surface of the prostate, and by cutting a fibrous adhesion here, using the handle of a scalpel to push back the rectum there, gradually uncover the prostate and work the rectum

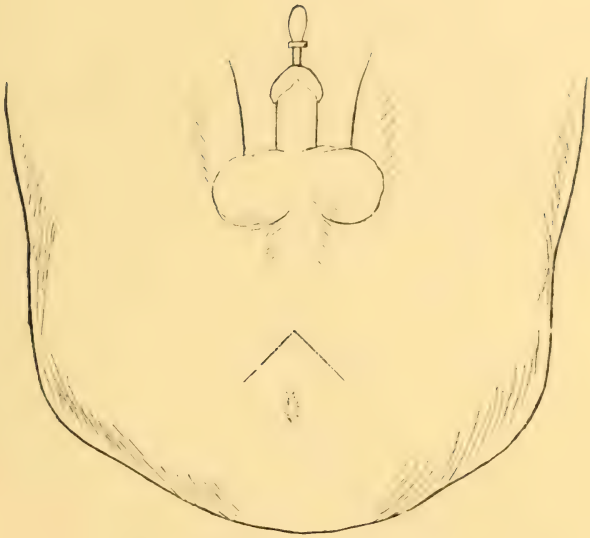

Fig. 1.—The inverted V-incision.

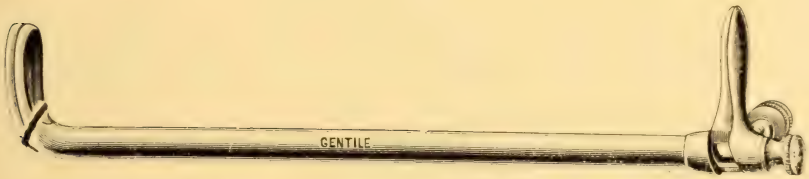


Fig. 3.—Prostatic tractor closed for insertion.

Fig. 4.—Tractor opened out (as after introduction).

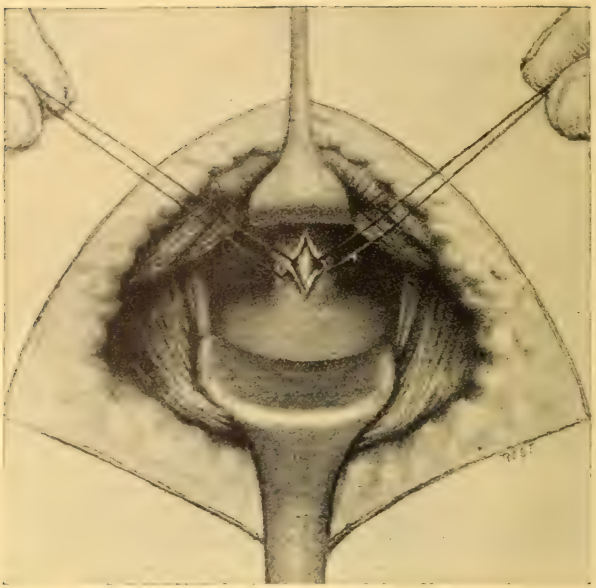

Fig. 2.—Incision of membranous urethra for introduction of tractor.

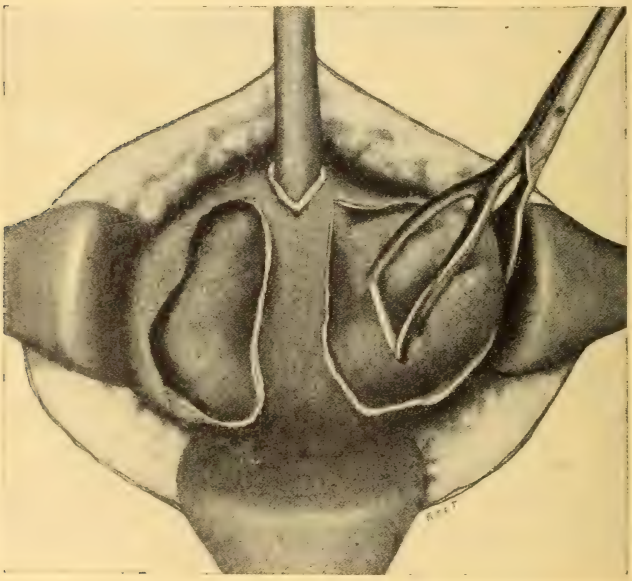

Fig. 6.—The enucleation of lateral lobes.

Fig. 5.—The prostate drawn down by tractor.

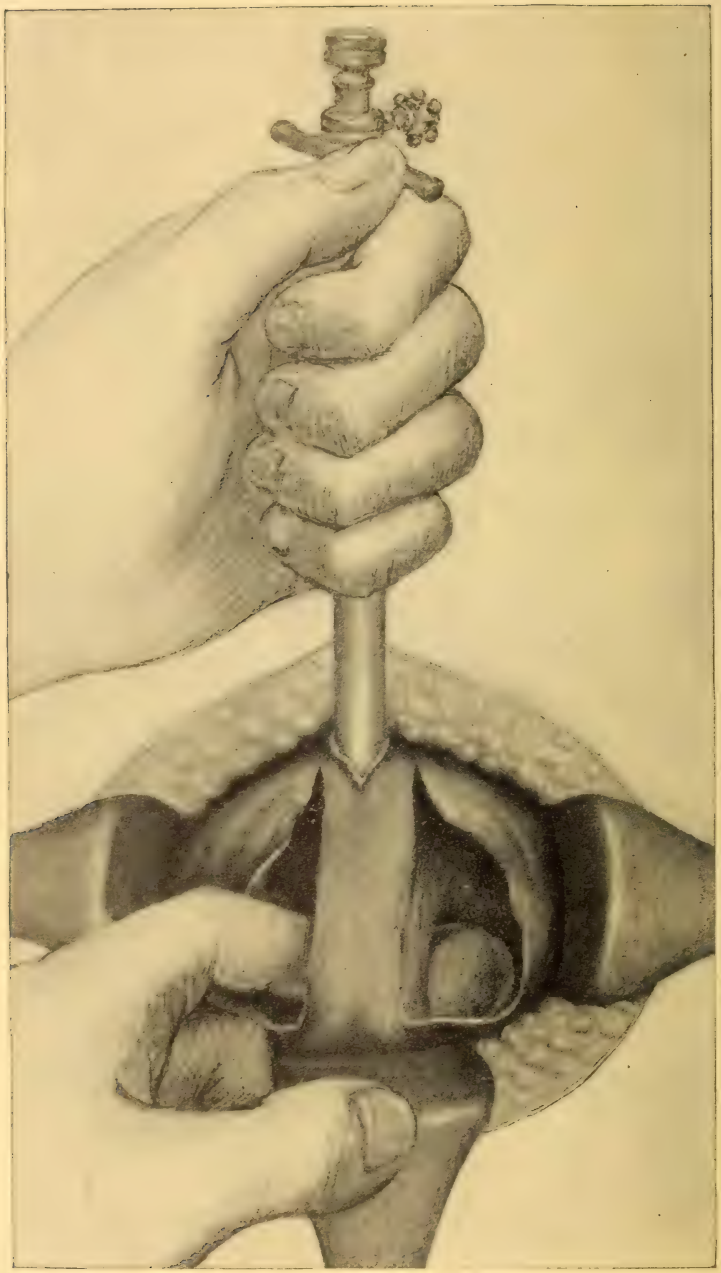

Fig. 7.—Delivery of the middle lobe into left lateral cavity.

backward. I am convinced that most rectal injuries are produced by pushing directly against the rectum rather than working along the posterior surface of the prostate. The use of the finger is particularly dangerous, as there is great temptation to go too fast, and it is no difficult matter to poke a hole with the tip of the finger into the rectum.

I have now freed the entire posterior surface of the prostate, and inserting this broad, sharply bent retractor have an excellent view not only of the prostate but the seminal vesicles above it (Fig. 5). You see that the capsule is fairly smooth but slightly irregular; the prostate feels hard and the lobes will not be enucleable as in the case of benign hypertrophies.

The capsular incisions which I am now making are a little more widely separated than in hypertrophy cases as I am particularly anxious to take no risk of injuring the ejaculatory ducts. (They are about as shown in Fig. 5.) These incisions are made about 1.5 cm. deep and the block of tissue between them contains the urethra and the ejaculatory ducts. Probably the most constant thing about the prostate is the position of the ejaculatory ducts, which lie in a pouch of fascia always immediately beneath the floor of the urethra, the position of which can always be determined by the shaft of the tractor. I have never cut into the ducts in making these capsular incisions. The statements which have been made by certain writers in regard to the impossibility of locating the position of the ducts are absolutely incorrect. There is no danger whatever of accidentally destroying the ejaculatory ducts or of producing impotence unless the operation be very carelessly done.

With this blunt dissector or enucleator I shall proceed to separate the capsule and urethra from the lateral lobes. You see here that the capsule is quite thin and strips easily from the outer surface of the lobes. The urethra is more adherent, but the depth of the primary incisions has done most of the work and the rest of the separation is not difficult, and the lobes are now free everywhere except anteriorly and

next to the bladder. Curved scissors are usually necessary to divide the anterior adhesions, after which I introduce my finger and complete the separation of the lobes. I find the deeper portion very adherent at the vesical orifice and it will be necessary to make use of the lobe forceps (Fig. 6) and free the deepest adhesions with scissors. During this procedure I entrust the holding of the tractor to an assistant.

The lobes are now removed and as you see are very little larger than in the case of a normal prostate. I know that I have left none of the tissue of the lateral lobes behind because with my finger I can easily feel the blade of the tractor with only thin mucous membrane intervening.

The cystoscope showed a small median bar; this is evidently so fibrous and adherent that it will be impossible to deliver it by means of the tractor into one of the lateral cavities as is usually possible in cases of prostatic hypertrophy (Fig. 7). I could, however, easily excise it through one of the lateral cavities without injuring the ejaculatory ducts beneath it or removing any of the urethral mucous membrane, but in this case I am anxious to inspect and treat the posterior urethra from which blood has been coming, and on this account I shall now open the posterior urethra along the left lateral wall which, of course, leaves the ejaculatory ducts still preserved, as you see. I use this same incision to remove large calculi from the bladder after prostatectomy, and no difficulty is encountered in extracting a stone over two inches in diameter. Examination of the floor of the urethra shows an enlarged hemorrhagic looking veru montanum and several ulcers are seen along the floor of the urethra. I am now excising the portion of this redundant veru montanum and shall curette the ulcers. The median portion of the prostate is definitely thickened. I can grapple it with forceps, and by means of the sharp periosteal elevators and scissors have no difficulty in removing it without the mucosa covering it and without destroying the ejaculatory ducts beneath it. Having removed the tractor I now introduce my finger into the bladder

and find the small portion of the median bar has slipped back into the bladder. I have no trouble, however, in grasping it with forceps and excising it. The vesical orifice is now large, but the sphincter is still to be felt around the finger. I can easily feel most of the bladder cavity; there are no stones present, no diverticula, no ulcers or tumor, and nothing to suggest tuberculosis.

I shall now insert these two large catheters which, as you see, have been divided obliquely at their ends and sewed together so as to form a common point to facilitate their introduction. These are introduced through the perineum into the bladder and the lateral cavities are packed each with a single strip of iodoform gauze. You see I have not attempted to sew up the divided lateral wall of the urethra as experience has shown that it heals perfectly well. Before closing I always insert my finger, which is now gloved, into the rectum and examine the anterior wall of this viscus. This is a very important procedure. You see that the rectum has not been torn into, but that the separation of the normally approximated levator ani muscles has left the anterior wall exposed. You see how thin it is, and how, without the support which is normally given it by the levators, it could easily be ruptured by straining at stool, and that if a gauze pack were placed against it here necrosis might easily result. Both of these can be prevented by simply drawing the two levators together by a single suture of catgut, as I am now doing. You see now how this thick mass of muscular tissue has been drawn together in front of the rectum, thus restoring the male perineum. Before adopting this plan I had several cases of rectal breakdown, due I believe to necrosis. I have now used this single suture of catgut to approximate the levators in 140 consecutive cases and have not had a single rectal fistula as a result. This one suture has entirely removed all danger of the only bad result which I feared after this operation.

I am now closing the skin by interrupted sutures of catgut, leaving a small opening in front for the escape of the tube

and gauze drainage. The tubes are fastened at the anterior angle of the wound with a suture of heavy silk. The bladder is now irrigated through one tube and out the other with salt solution. You see there is very little hemorrhage. On return to the ward this irrigation will be continued for several hours, until the fluid comes away entirely clear and all danger of the tubes becoming blocked by clots is removed. Using a two-gallon tank very little difficulty is experienced in keeping up this irrigation, the inlet tube being clamped off so that only a small stream of fluid is allowed to enter, unless the hemorrhage be profuse. The patient is generally given an infusion either on the table or after return to the ward. The gauze pack will be removed to-morrow morning and the tubes in the afternoon. We generally have the patient in a wheel-chair on the second or third day and instruct him to walk as soon as possible. He will remain in the hospital about three weeks, though some cases leave earlier. As a rule the fistula closes during the second or third week. In several cases no urine has escaped through the perineum after the first week. There is practically no danger of a persisting perineal fistula. Sometimes when the patient is very feeble or the bacterial urinary infection very severe the closure may be slow, but if it is occasionally curetted (a gimlet curette has been found very satisfactory) and cauterized with nitrate of silver the fistula always heals. Even while still present only a few drops of urine escape during urination and cause the patient very little discomfort. No sounds are ever passed after the operation, as it has been shown that we have no stricture to fear. We usually pass a silver catheter before the patient leaves the hospital so as to demonstrate that no residual urine is present.

In the operation which you have just seen the prostate has been exposed almost entirely by blunt dissection, only two insignificant median-line structures having been divided. The retraction of the levator muscles has been sufficient to thoroughly expose the field of operation. This is sufficient

for the removal of very large hypertrophies or for excising a carcinomatous prostate in its entirety with the seminal vesicles and neck of the bladder, as has been carried out by me in 6 cases.[1] The advantages of this technique are that no important muscular, sphincteric, or sexual structures are destroyed, that the triangular ligament and its sphincter and the bulb overlying it are left intact, incontinence of urine thereby being avoided. The preservation of the urethra and the use of only a small linear urethrotomy in the membranous urethra back of the sphincter remove all fear of stricture. With proper retractors an excellent view is obtained, so that you see what you are doing, and the prostatic tractor allows you to draw down at will any portion of the prostate and remove it without any of the mucous membrane covering it without injury to the ejaculatory ducts, none of which is possible with suprapubic prostatectomy or a median-lined perineal prostatectomy done in the dark.

We have very carefully followed our cases and have demonstrated that the operation if properly performed is always curative of the obstruction to urination; that it does not affect the sexual powers, and in fact, in some cases in which the sexual powers are weakened or absent, furnishes a restoration to normal vigor. (This is probably explained by the removal of pressure upon the ejaculatory ducts and veru montanum.) The mortality is *nil*. During the past three years the mortality rate has rapidly decreased, and in the last 110 cases there have been but 2 deaths, the last 68 cases being without a single death, every patient leaving the hospital well.

The remarkably good results which have been obtained in hypertrophy cases have led me to employ it more and more in cases of rebellious prostatitis with or without obstruction to urination, and I have now had about a dozen cases with excellent results. The sexual function—which is very impor-

[1] See complete report Johns Hopkins Hospital Reports, vol. xiv, and these Transactions, vol. xviii.

tant in these—has not been impaired, and the discomfort which was caused by the persistent prostatitis has been removed.

I, therefore, believe that the perineal route is by far the preferable one for all forms of prostatic surgery, whether chronic prostatitis, prostatic hypertrophy, or cancer; the great advantages being that you see what you are doing, that the exact nature of the disease can be determined and the appropriate operation performed, and that in benign cases no important structures are destroyed.

# VOLVULUS OF THE SIGMOID COLON; RESECTION; RECOVERY.

By Joseph C. Bloodgood, M.D.,
*Baltimore.*

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This specimen, a giant sigmoid colon, the seat of recurrent volvulus, was removed from a white male, aged sixty-five years, on August 2, 1906.

The distention of the colon began at about the junction of the descending colon with the sigmoid. This distention is not abrupt. It continues the entire length of the sigmoid up to the rectum; here, although less in diameter, it continues to the anus. The lower portion of the rectum is not in the specimen, because during the resection this portion of the rectum was left behind. There is no evidence of obstruction to the lumen of the removed portion nor in that portion of the rectum not removed. Beginning at about the middle third of the sigmoid colon and extending to where the rectum loses its peritoneal covering, the visceral peritoneum is thickened by chronic inflammation. The "foot points" of the sigmoid are very near together, and there is a chronic inflammation of the mesentery of the sigmoid. These two factors are considered of etiological importance in the cause of volvulus. The mesentery was not unusually long. At the operation there were no adhesions and this giant sigmoid could be lifted out of the abdominal wound and placed on the skin of the abdomen for resection without tension.

Clinically the case is of interest because it represents numerous recurrent attacks of obstruction. The patient was

first admitted in 1890, sixteen years ago, with his first acute attack. The abdomen was opened and the large volvulus of the sigmoid reduced. There was a free interval of two years, and at the second admission, in addition to reducing the volvulus of the sigmoid colon, a band was divided. Since 1892, a period of fourteen years, the patient was admitted to Dr. Halsted's clinic thirty-one times because of symptoms of obstruction. The condition was relieved by high enemata. Operation was suggested to the patient on account of the increasing frequency of the attacks.

The resection and lateral anastomosis was made between the ascending colon and rectum. At the present date (February, 1907), five months since the operation, the patient has had no symptoms of obstruction. This is the longest interval without an attack for a number of years. He informs me also that his general health is very much improved. Previous to the operation he had symptoms suggesting a chronic autointoxication, headache, malaise, poor appetite, and a general feeling of depression. All of these symptoms have disappeared and he has gained in weight.

# SURGICAL TREATMENT OF ANEURYSM; PLASTIC OPERATION ON THE NOSE; GIANT COLON; SUCCESSFUL LIGATION OF THE INNOMINATE ARTERY.

By J. M. T. Finney, M.D.,
*Baltimore.*

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## SURGICAL TREATMENT OF ANEURYSM.

I am sorry a misunderstanding has occurred with reference to the wiring of this aneurysm. The case is not a suitable one, and so, instead, I shall speak briefly of our method of procedure.

A few years ago some experimental work was done by Dr. Guy L. Hunner, of this city, in regard to wiring aneurysms. In fact, I may say that all of the really important work that was done here in this respect was done by Hunner. My part in it was the clinical application. I have wired nine aneurysms, I think, in all.

Dr. Hunner published a full account of his work in the *Johns Hopkins Hospital Bulletin* for July, 1900. He reviewed the literature and reported all of the cases that were accessible up to that time. There were twenty-three cases, with 17 per cent. cures. Of these, 75 per cent. were thoracic and 25 per cent. abdominal aneurysms. There is an element of danger in this operation. We have had two cases with fatal results, which we believe were directly traceable to the operation.

Personally, I have operated on eight or nine cases of thoracic and abdominal aneurysms. In the the failures were more

than the successes. In one of the cases (a medical man) I wired a thoracic aneurysm; he subsequently went to his home, and within a year had a case of aneurysm in his own practice which he wired after the method used upon himself. That patient, the Doctor wrote me, suffered from gangrene of the arm, so that subsequently amputation became necessary.

There had been many methods suggested for the treatment of these hopeless cases up to the time of the Moore-Corradi method, but this method offered a better chance for recovery than any other with which I was familiar. We used it in suitable cases that presented.

I have had two apparent cures, but they did not last. One patient was captain of a vessel. For some months he did fairly well, but within a year from the time of the wiring of the aneurysm he died suddenly at sea. The cause of death was unknown. The physician whose case I spoke of remained well enough for six or eight months to attend to his professional work. While there was some local pulsation, all the symptoms had disappeared. I do not call that case a cure, but one in which marked relief was obtained. Within a year, however, he developed symptoms again, and died of an intercurrent pneumonia. No autopsy was held. In two or three other cases amelioration of the symptoms has been quite marked.

The method is simply this: In 1864 the idea of introducing a permanent foreign body into the aneurysmal sac was first suggested by Moore. He inserted wire to the extent, I think, of fourteen feet in his first case, and that patient died of sepsis. The principle, however, was a good one. Corradi treated an aneurysm by sending an electric current through it, and since that time a number of cases have been reported in which that method was pursued. The method suggested by those two men was elaborated by Stewart, of Philadelphia.

As to the question of the metal to be used for wiring, Hunner found in his experimental work that the softer the material the better, because of the corrosion of the electric current in

whipping out the fibrin of the blood. Therefore, after experimenting with various metals and alloys, he used 75 per cent. copper to 1000 silver. This is wound tightly upon a spool, and by further experimentation he found eleven feet was the approximate amount that could be used. It is put on a spool and coiled, so that it may help to fill the cavity of the aneurysm. If it is not coiled tightly, it may go in any direction, and if it should strike the wall somewhere, it will double upon itself. You can pass it into a needle readily, and when coiled it fills the sac satisfactorily.

After many trials he found that French lacquer, which he got from New York, was the most satisfacory material with which to insulate the needle. This will not stand boiling, but it does stand dry heat. The tip of the needle is not covered with the lacquer, so it goes through the sac much more readily than when it is lacquered at the point. This prevents the current from being disseminated in the sac or possibly in the interior of it. These needles are of various lengths. For a thoracic aneurysm one needs a shorter needle.

Some experiments were made as to the strength of the current. Dr. Hunner found a weak current long-continued, gave a better clot than a strong current which was used for a short time. In using the electric current one should be sure that the positive pole is always in the aneurysmal sac, and not the negative pole. Abbe, of New York, had a disastrous result from alternating the poles. Dr. Hunner found by experiment that the negative pole disorganized the clot and softened it, so one should be sure that the positive pole is in the sac of the aneurysm. With ten milliamperes for an hour, a better clot is formed than with a stronger current for a shorter time. The advantage of a weak current is that you run no risk of burning the sac. I used too strong a current in my first case and burned through the sac.

## PLASTIC OPERATION ON THE NOSE.

This case is interesting. It is the second one of the kind I have had. I show you a photograph of the patient which was taken before the operation. The case is one of congenital lues. The nose, instead of being convex, was concave. There was simply a hole, with no projection whatever. In the case before us there was a small opening on one side, corresponding to the nostril, and a little bit of skin projected forward on one side. We had absolutely no bony material to work on. The question was how to get a framework for the soft parts. In the other case I came to the conclusion that a finger was the natural solution of the question. This young man had a baseball finger with the phalangeal joint of the ring finger of the right hand stiff. I took his baseball finger with which to make a nose. These are the steps of the operation: First I removed the nail; denuded the entire circumference of the end of the finger; denuded the dorsum of the finger up to and a little beyond the first phalangeal joint; then dissected loose the skin of the nose, which was puckered and inverted. By careful dissection I freed it, separating it from the inside. I denuded the under surface; took a blunt instrument and passed it underneath the skin and separated the skin from the nasal process of the frontal bone. I had a raw surface on the dorsum of the finger and a raw surface on the tip; then I inserted the finger up the nose (indicating), so that the tip of the finger rested on the nasal process of the frontal bone; then I attached the free border of the nose to the free border of the denuded portion of the dorsum of the finger, and fixed the arm in this position (indicating) with plaster for two weeks, at the end of which time I amputated the finger.

In this case I used the little finger. It was considerably smaller than the other and makes a little better result than if we had used a larger finger. I amputated the finger at the end

Before.

After.

of two weeks. We had the first phalanx of the finger protruding from the nose; then, after waiting two weeks until we were sure the circulation in that was good, I flexed the finger at right angles, and inserted the free border of the finger into an incision over the nasal process of the superior maxilla, thus having raw surface against raw surface. I was careful to get bone against soft parts. She has a freely movable nose. This case has been under observation some five weeks, and I have not had time to complete the work.

In both cases the lip was retracted, as you see in this case, at quite an angle, so I may have to fill out this rather ugly angle. What I contemplate doing is to take out a diamond-shaped piece from either side of the lip, under cocaine, making the columna of the proper width, freeing that at its attachment here. The lip is too wide to pucker properly, and if we bring the columna in at the proper horizontal angle, at the next sitting, we will have quite a satisfactory nose. You can feel the finger, which makes a solid bridge for the nose.

There is one interesting feature about the first case which I desire to mention. I thought I had removed the matrix of the nail completely, but found I had left just a little corner, and now, when he cuts his finger-nails, he has to cut the spicule between his eyes as well.

## GIANT COLON.

This case is of interest more from a pathological than a surgical point of view. It is a case of giant colon. This patient came to the hospital with a diagnosis of tuberculous peritonitis. He had an enormous colon. I am not exaggerating when I say the diameter of the colon was five or six inches. Beginning at the hepatic flexure the dilatation extended around the transverse and descending colon to about the brim of the pelvis, where it abruptly stopped. It began

as abruptly as it stopped, without the slightest suggestion of an obstruction of any sort.

I will not take the time to go into the pathology of this condition.

Many suggestions have been offered, none of which has been satisfactory and none explanatory of every case; and of all the cases reported, some of them are due to obstruction, some are congenital, and some are acquired; some are due to intrinsic, and some to extrinsic, obstructions. In some of these cases, as in this particular instance, there is a thinned condition of the abdominal wall due probably to nervous influences. In this case we have a well-defined condition which, so far as I can find out, has not been noted in other cases. In other words, we had here lymphangiectasis of the mesentery. The dilated blood-vessels in it were greatly hypertrophied, as were also the lymphatic glands. I removed a section, which showed nothing but hyperplasia. In other words, this was a disturbance of nutrition, pure and simple. Here we had a superabundance of nourishment to that particular segment of the intestine and consequent hypertrophy as a result. I believe the explanation in this case will suffice for a good many of the other cases.

As to the treatment, the first thing was to relieve the symptoms of obstruction. I followed simply the usual routine in making a colostomy, and left the wound open for six months. The symptoms subsided quickly and satisfactorily. At the end of that time the patient had gained perceptibly in weight, and improved in color. He was very much run down when he came in.

Of the variout suggestions that have been offered for treatment, the most satisfactory one apparently has been the excision of the entire growth. Inasmuch as the patient had lived for six months in a fairly comfortable condition with this enlargement in his belly, it impressed me as being unnecessary to subject him to a very serious operation. Therefore, I simply short-circuited the large intestine by entero-anastomosis,

anastomosing the hepatic flexure with the sigmoid, and he has done well since. He has regular and satisfactory bowel movements now. I have been keeping him under observation, waiting to see what would happen, and if he has any trouble or discomfort I shall not hesitate to resect this portion of the bowel later.

### SUCCESSFUL LIGATION OF THE INNOMINATE ARTERY.

I wish to show you a case in which we successfully ligated the innominate artery. The operation was done six months ago for a large aneurysm of the first portion of the subclavian. This is the ninth successful case. This is an American operation. It was done first by Mott, and first successfully by Smythe, and of the nine successful cases four are American operations, and, curiously enough, of the last seven ligations six have been successful, so it is getting to be an operation that is not attended with as large a mortality as formerly.

A point of interest in this case was the exposure of the innominate. I simply took a rongeur forceps, gnawed away the junction of the clavicle and sternum, and by pressing the soft parts back with my fingers, in a few minutes I had a very satisfactory exposure without difficulty. Fifteen minutes was saved in so doing. As soon as I had tied the innominate, pulsation in the aneurysm ceased. My assistants and I noticed it. I started to close up, when my assistants noticed the aneurysm beginning to pulsate again. Pulsation was almost as pronounced as before the innominate had been tied. To be sure that the ligature was all right, I opened the wound again, examined the ligature, and found it was tight. What should I do? It was a question whether I should ligate the carotid or ligate below the aneurysm. It seemed to me that the most satisfactory and simplest procedure, one that would be followed by less disturbance, would be to go below the aneurysm and ligate. Accordingly, I extended the incision and

ligated the axillary artery just below the clavicle. Pulsation was considerable. I thought more than likely that after a little while, the sac being closed, pulsation would cease, and the sac would fill with a clot. It did. After five days, pulsation not only diminished, but ceased entirely, and a satisfactory cure was effected. My idea was to wire the aneurysmal sac later, if this did not afford relief. There was no appreciable disturbance of the circulation except for the first few hours. He is now able to move his arm satisfactorily, and the aneurysm has been steadily diminishing in size until now. I have not seen him for a few months. The aneurysmal sac is one-half less now than at the time I performed ligation, and the patient's physician tells me that since that time it has diminished still more.

Thirty-six cases of aneurysm have been operated on, and of this number 28 have died, a mortality of 78 per cent.

# THE *X*-RAY DIAGNOSIS OF THORACIC ANEURYSMS.

By F. H. Baetjer, M.D.,
*Baltimore.*

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In the ten years that have elapsed since the discovery of the *x*-ray all branches of medicine have been benefited by this new diagnostic agent. Upon no one branch of medicine, however, has as much light been thrown as upon the chest and its diseases; especially true is this of the heart and its appendages.

Heretofore with the means at our disposal the diagnosis of aneurysm was often difficult and sometimes impossible. In the very early cases, just when the most good could be done, aneurysms would be overlooked and probably not recognized until their size would preclude almost any hope of doing good. Especially true is this in strong, muscular men at about the fourth decade of life, who have degenerative processes in the walls of the arteries due to syphilis or alcohol. On the other hand, a diagnosis of aneurysm may be made which would of necessity cause an absolute change in a patient's habits and mode of living, and yet the subsequent history or possibly the autopsy would show that all the patient's care and anxiety were needless, as no such pathological condition had existed. Since the discovery of the *x*-ray this doubt and uncertainty have been removed, and now we can practically make an absolute diagnosis, both positive and negative.

The two methods at our disposal, radiographic and radio-

scopic, give us an accurate knowledge of the chest and its organs. Whereas the radiograph gives us a permanent record and reveals more detail, yet the examination by the fluorescent screen gives us a more comprehensive knowledge of the chest. By means of it we can examine the chest from various angles and see the actual pulsations of the heart and its vessels. In this hospital the value of this method of examination has become so apparent that it has become practically a routine practice to examine thus every obscure chest disease.

Briefly, the method of procedure here is as follows: Before proceeding with the *x*-ray examination the patient is examined to see that there are no abnormalities as to the formation of the chest or spine due to some disease. For example, a diseased vertebra may push the aorta aside and the condition simulate that of an aneurysm. In rachitic children it is common to see a pulsating tumor to the left of the sternum simply due to the displaced arch of the aorta.

Having assured ourselves that no abnormalities exist, the patient is placed in a sitting position, with his back to the tube. The tube is placed at the level of the third rib and from twenty to twenty-four inches away. At this distance there is very little divergence of the rays, and consequently the image projected upon the screen is not materially enlarged or distorted.

After this examination has been made, the tube is placed in front at the same level and the patient is examined from the back. By means of these two positions we can locate the position of an organ by comparing the size of the shadows in each case, since the shadow is always larger the further it is removed from the screen. Now, by means of transverse, or slightly transverse, illumination, we can frequently tell whether the object has its attachment in front or behind. This is especially important in determining whether an aneurysm springs from the ascending or descending portion of the aorta.

Before leaving the chest it is well to make an examination

both from the front and behind, with the tube placed on a level with the end of the sternum. By having these fixed positions of examinations we get a composite normal, and one soon learns to tell any variation from this normal.

The objection may be advanced that this method does not give an absolutely true picture of the chest, because the tube and screen are not always in the same plane. By making the examinations at two different levels the objection is obviated and our composite picture is a true one, as the records of the physical examinations and autopsy reports in this hospital have shown.

In a normal individual the fluorescent screen reveals the following picture: The lung substance, being permeable to the ray, gives us a clear view of the clavicles, scapulæ, and ribs. In the median line we have a dark band about two inches broad, extending downward from the clavicular articulations to be lost in the shadow of the heart. This is caused by the spine and sternum. On the left side, low down, the pulsating heart can be readily made out, its shadow fusing into that of the diaphragm. On the right side is a slight pulsating shadow due to the right heart, and beneath, the shadow of the right diaphragm. Since the aorta lies just above the spine, its shadow is obscured by that of the spine and sternum and consequently no pulsation can be seen either to the right or to the left of the sternum. This practically always holds true. In very thin individuals, however, a slight pulsation of the arch may be observed occasionally, but this is rare. In only 1 per cent. of the cases has this been observed. Abnormalities causing displacement of the aorta have already heen mentioned.

From what has been said, we can assume that any shadow existing either to the right or to the left of the sternum is abnormal. This may be due to a variety of conditions, such as:

1. New growth.
2. Enlarged glands.

3. Displaced aorta.
4. Dilatation of the aorta.
5. Aneurysm of the aorta.

In new growths and glandular enlargement the history of the case is important and then, too, the shadow cast is more dense than one cast by an aneurysm. Absence of pulsation is of value, but cannot always be relied upon, as frequently we get a transmitted pulsation which can scarcely be distinguished from that of an aneurysm. The two distinguishing features, however, are that the shadow is darker and the edges are more hazy and irregular.

A displaced aorta can be ruled out by examining the spine and chest to see that there are no abnormalities.

In dilatation of the aorta a distinct pulsation of the shadow can be seen, but the diagnostic point is that the shadow disappears between pulsations because the aorta contracts markedly in this condition and its shadow lies within that cast by the sternum and spine. In this connection I may say that especially in such cases is the examination by the fluorescent screen of more value than by the radiograph. The radiograph may show no shadow if taken between the pulsations, or, if taken during the pulsation, show one. In either case a false interpretation of the condition would be made. Aneurysms, where the sac is large and pulsating, offer no difficulties as to diagnosis.

The detection of small aneurysms just when an early diagnosis is of such value to the patient is often difficult. The one point to bear in mind is that a pulsating shadow which does not disappear between pulsations should always be viewed with grave suspicion. Very small aneurysms can be diagnosed, as evidenced by Case 74, which we will consider later.

Broadly speaking, aneurysms may be divided into two classes, namely, sacculated and diffuse. The sacculated ones offer but little difficulty of diagnosis. The shadow pulsates and stands out as a rounded mass from the sternum.

Diffuse aneurysms, where the aorta is uniformly enlarged, are marked by a broad shadow extending down along the sternum, generally on both sides. The distinguishing feature, as spoken of before, is that the shadow persists between pulsations.

Naturally, the appearances vary greatly and depend upon the size and position of the aneurysm. Roughly speaking, the positions may be classified as follows:

1. Aneurysm of the ascending portion of the aorta usually casts a shadow more to the right than to the left of the sternum, above the heart, and by localization would be found to be nearer the anterior than the posterior wall of the chest.

2. Aneurysm of the arch casts a shadow slightly to the left of the sternum, and this shadow extends well up into the neck and by localization would be found nearer the anterior chest wall.

3. Aneurysm of the descending arch of the aorta casts a shadow to the left of the sternum, and by localization is generally nearer the posterior than the anterior chest wall.

The above classification covers the common situation of aneurysms, though they may occur anywhere in the course of the thoracic aorta. The records of the Johns Hopkins Hospital show one case where the aneurysm arose from the aorta just before it passed through the diaphragm. The heart was displaced upward and outward, and just below to the left of the median line a large pulsating mass the size of an orange could be seen readily. The movement of the left side of the diaphragm was markedly inhibited. The interesting feature in this case was the fact that the patient had been sent to the hospital as a case of rheumatoid arthritis of the spine.

Since practically all obscure chest conditions are examined in the Johns Hopkins Hospital as a routine procedure, we will not take into consideration those cases in which no aneurysms were found, but will confine ourselves entirely to those of aneurysms.

In the past four years there have been 104 cases in which a positive diagnosis of aneurysm has been made. These cases extended over a wide field; some were found by accident, in others the signs were suggestive, and in still others both signs and symptoms pointed distinctly to aneurysm.

A brief mention of some of the unusual cases may be of interest to this Society.

Case 68 is of interest on account of the absence of physical signs and a previous diagnosis of intercostal neuralgia. The patient was a man, aged forty-five years, who complained of pain in the left side and loss of weight. The onset of his illness was one year ago, with pain in right lower thorax radiating to the spine, worse on exertion, and paroxysmal in character. This pain gradually increased in intensity and frequency of attack. He entered a hospital in one of the Southern States and there his disease was diagnosed intercostal neuralgia.

After he left the hospital his symptoms increased, with the addition of weakness and some dyspnea. He came to Baltimore to consult Dr. Osler. The physical examination was absolutely negative for aneurysm. Dr. Osler referred the patient to me for an *x*-ray examination, and a large, well-defined aneurysm of the ascending aorta the size of an orange was discovered.

Case 71, woman, aged twenty-three years, complained of shortness of breath. Onset of trouble five weeks ago with cough and shortness of breath. Symptoms increased, accompanied by severe attacks of dyspnea. The patient was admitted to the hospital in the service of Dr. Osler. At that time marked respiratory distress and cyanosis. Physical examination of chest negative, except for harsh breath sounds due to labored breathing. Diagnosis: Some obstruction to trachea, either external or internal, due probably to a new growth.

An *x*-ray examination was made and a small aneurysm of the transverse arch of the aorta springing backward and causing tracheal pressure was found. The patient improved

for a time, when she suddenly became worse and was unconscious, due to asphyxia. She was immediately seen by Dr. Halsted and, as the obstruction seemed below the larynx, he did a tracheotomy and introduced a rubber tube to a point 15 cm. below the tracheal wound, which gave immediate relief. Exploration of the mediastinum was deemed advisable. A portion of the sternum and ribs was removed and the mediastinum was well exposed, but a hasty exploration revealed no tumor. The patient died that afternoon and an autopsy showed a very small aneurysm coming from the transverse arch, going backward and pressing on the trachea.

Case 85 illustrates the bad results of a late diagnosis. The patient, a man of sixty-five, complained of shortness of breath and respiratory distress. His illness extends over a period of ten years. He was seen seven years ago by a prominent consultant who diagnosed the case as one of asthma. He never improved to any extent. Two months ago his voice became husky and indistinct, due to a recurrent laryngeal paralysis. With the exception of the asthmatic condition, his chest was negative. An *x*-ray examination revealed one of the largest aneurysms I have ever seen. It practically filled the entire upper left chest.

Case 100 is interesting on account of the peculiar symptoms. A male, aged fifty-seven years, complained of pain in back, side, stomach, and chest. Three years ago he had a gall-stone operation to relieve these symptoms. No gall-stones were found and his symptoms did not disappear. One year after the operation shortness of breath came on. He came to this hospital and the marked pain and rigidity of spine suggested spondylitis deformans.

The physical examination of the chest was practically negative. There was a slight pulsation in the second intercostal space which was attributed to an enlarged mammary vessel. To rule out aneurysm definitely, however, the patient was referred to the *x*-ray department, where a large aneurysm of the descending portion of the aorta was found.

The question naturally arises, in how many of these cases has the subsequent history proved the diagnosis of aneurysm to be correct? Of these cases 70 to 75 per cent. were referred to the *x*-ray department by the medical side with a definite diagnosis of aneurysm. In these the position and size of the tumor as shown by the *x*-ray findings practically always agreed with the medical findings; in 20 to 25 per cent. the diagnosis was doubtful, but in many of the cases the subsequent history proved the *x*-ray diagnosis to be correct. In 5 per cent. of the cases the aneurysm was found by accident, there being no physical signs.

Of all of these cases 18 per cent. came to autopsy, and the anatomical findings corresponded exactly with the *x*-ray findings.

Naturally, in the class of cases that come to a hospital, many of them are finally lost sight of and final proof of the diagnosis cannot be obtained.

The value of the *x*-ray examination is so apparent in such conditions that it needs no one to urge its claims. Dr. Osler believes that an *x*-ray examination of the chest should be made in all cases.

In conclusion, let me quote Dr. Williams, who sums up the situation so admirably: *X*-ray examinations should always be made. Normal outlines in the upper part of the chest give us the best assurance that an aneurysm of the aorta is not present, though symptoms may obtain which lead the physician to suspect it. On the other hand, if an aneurysm be present, its outline will be demonstrated by the *x*-ray examination. An outline suggestive of aneurysm may be due to other causes, as new growth, for example. But confusion of this kind is not common and we can in most cases convince ourselves by a careful examination whether or not it is an aneurysm which casts the shadow. *X*-ray examinations enable us to determine the extent of an aneurysm more accurately than the usual physical examination, and to detect its existence at a much earlier stage. Successive examinations

enable us to determine whether or not it is increasing in size.

"To make a definite diagnosis of aneurysm by the usual physical examination we may be obliged to wait for the development of marked signs and this delays treatment. On the other hand, if the physician begins treatment because the signs are suspicious, he runs the risk of subjecting his patient to unnecessary regimen. The advantages of *x*-ray examination when compared with the usual physical examination are evident. A definite diagnosis can be made in most cases before there are physical signs. Treatment can, therefore, be begun at an earlier and more hopeful stage, can be planned more intelligently as the knowledge of the position and extent of the aneurysm is more accurate, and its results can be better estimated because we can more accurately measure any change in size."

# SURGERY OF THE LIVER.

## SERIES OF LIVER CASES IN WHICH IT WAS NECESSARY TO REMOVE A SEGMENT OF THE LIVER OR TO SUTURE TORN AREAS.

By Thomas S. Cullen, M.D.,
*Baltimore.*

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In the *Journal of the American Medical Association,* April 22, 1905, in a brief review of the literature on the surgery of the liver, I emphasized the fact that, although Brunn, in 1870, had removed a small hepatic tumor, the surgery of this organ had attracted little attention until 1886, when Linn successfully performed a partial resection. Between 1886 and the date of my paper, over 100 resections of portions of the liver had been reported.

In this review, reference was made to the excellent work of Keen, Tiffany, and others in America, of Terrier and Auvray in France, and of Langenbuch in Germany, as well as to the various animal experiments that had been performed. The various methods of suturing the liver were illustrated, and the great value of Kousnietzoff's liver needles pointed out. Since the publication of my paper, in two cases in which I had found it necessary to remove wedges of the liver substance, I approximated the raw surfaces by means of blunt needles; and in two other cases, where the liver had been lacerated, I readily brought the friable surfaces together and completely checked the bleeding by the use of the blunt needles. In all of the five cases the outcome, as far as the operative technique was concerned, was eminently satisfactory.

REMOVAL OF A PORTION OF LIVER.

Case I. Removal of a large secondary carcinoma of the liver.

Case II. Hysterectomy for a large myoma; excision of the gall-bladder and removal of a wedge of liver substance, where carcinoma of the gall-bladder could not be excluded.

Case III. Excision of the gall-bladder and a wedge of liver substance, where there was great thickening of the wall of the gall-bladder and a suspicion of malignancy.

A reference to the first case will show that a secondary carcinomatous nodule, 16 x 14 x 13 cm., was cut away from the lower portion of the liver and that the raw surfaces were approximated with little difficulty. It will also be noted that very little blood was lost during the operation, and that although the patient had only one kidney, she promptly recovered, despite the fact that at the time of operation the urine from this remaining kidney contained blood and pus. It would be difficult to find a case offering less chance for a successful liver resection. It is now over two and a half years since this liver tumor was removed, and although there is a definite local return where the left kidney was taken away, the liver appears to be free from any new growth.

In our second case, operated upon in the Hamilton General Hospital, after removing a myomatous uterus which extended far above the umbilicus, we found the enlarged gall-bladder filled with stones, and in its upper portion a densely thickened area, strongly suggestive of carcinoma. In this case, after liberating and clamping the cystic duct, I removed the gall-bladder and with it a wedge of liver substance. The bleeding was not excessive, and not having liver needles I used a bodkin, which can be readily obtained in any hospital or home. This case afforded, also, a rather severe test of liver surgery. The patient was a stout woman. She had been suffering for years from transitory glycosuria, and her urine

contained some albumin and casts. Moreover, a tedious hysterectomy was performed before the liver complications could be dealt with.

In Case III the patient was thin and there were no other complications. The omentum was densely adherent to the gall-bladder and the walls of this viscus were much thickened. We strongly suspected carcinoma, and even after the gall-bladder had been removed it was impossible to exclude carcinoma entirely.

### INDICATIONS FOR RESECTION OF A PORTION OF THE LIVER.

It is important to determine: (1) Whether the growth be malignant or not; and (2) if malignant, whether it is primary or secondary. In the case of metastatic neoplasms, as a rule, operation is contra-indicated, inasmuch as they are usually multiple. Primary carcinoma or sarcoma of the liver is rare. Should the hepatic growth be primary, the glands along the vessels and at the base of the liver should be carefully palpated to ascertain whether they have become involved. This point was first insisted upon by Tuffier. In cases of lymphadenoma with secondary nodules in the liver, operation is not advisable. In all cases of liver growths the following points should be carefully considered: (*a*) Whether the growths be single or multiple: (*b*) the situation; (*c*) the size; (*d*) whether they are pedunculated or not.

If the growths are multiple and scattered over a wide area, removal is difficult or impossible. On the other hand, provided that they are near the lower edge of the organ, resection is feasible. In fact, in the majority of cases, the situation is all-important.

The size is of little importance when the growth is pedunculated and situated near the lower margin of the liver. In the case of a large tumor, however, located near the middle of the organ, removal is exceedingly difficult or impossible.

If it is located near the edge, amputation is not difficult; but if it is near the middle of the lobe, a very large portion of the liver must be resected; otherwise a cavity is left which may be the source of an alarming or even of an uncontrollable hemorrhage. When the growth lies near the portal vein or hepatic arteries, operation is contraindicated, since large branches of these vessels are liable to be injured, with a resulting necrosis of correspondingly large areas of liver substance and the death of the patient. If the growth be pedunculated its removal is easier, inasmuch as the liver substance forming the pedicle is much tougher than other portions of the organ. This toughening is due to partial atrophy of liver-cells.

## REPORT OF CASES IN WHICH PORTIONS OF THE LIVER WERE REMOVED.

*A large carcinomatous tumor of the liver; removal seventeen months after nephrectomy for carcinoma of the left kidney (Figs. 1, 2, 3); temporary recovery.*[1]

I saw this patient in consultation with Dr. E. R. Trippe, of Easton, Maryland, on June 6, 1903. She was thirty years of age and of very slight build. For six months she had felt a small lump in the left side, but had hesitated to speak of it until a week previously. The lump was very noticeable in the region of the left kidney, and at first had increased in size only gradually, but later at a much more rapid rate. The patient had been operated upon on January 8, 1903, and we had found a very large carcinomatous kidney measuring 20 x 13 x 12 cm. The capsule of this was very delicate, and ruptured during the removal of the tumor, so that I did not think that the entire growth had been removed.

Histological examination of this kidney showed the growth to be a typical adenocarcinoma. The patient made a satisfactory recovery, and left the hospital after the fifth week.

[1] Abstracted from Jour. Amer. Med. Assoc., April 22, 1905.

On March 9, 1903, I received a letter from her saying that she was very much improved; the wound had healed entirely, and she was feeling much stronger.

*Subsequent History.* On June 2, 1904, I received a message from Dr. Trippe, saying that the patient was not so well and her temperature was 102.5°. Five days previous to this the patient had noticed a small lump just below the free edge of the ribs on the right side. At first it gave her no concern, but apparently doubled in size within a few days.

*Examination.* When I saw her the tumor occupied the region between the edge of the ribs and the anterior superior spine. It was round, tense, and somewhat movable. We were very careful in our examination, fearing that it might be a friable secondary growth following the removal of the large carcinomatous kidney in January, 1903. Her pulse was rapid; the temperature was 102.5° F.

*Operation.* As she was losing ground rapidly an immediate operation was decided upon. An incision having been made over the tumor it was found to be perfectly smooth and free from adhesions. The incision was continued upward to the free margin of the ribs and downward almost to the anterior superior spine. The growth had originated in the liver. It was larger than a child's head and almost globular in form. The edge of the liver appeared as a slightly elevated ridge, forming a crescent over the centre of the anterior surface of the growth (Fig. 1). Attached to the inner side of the growth was the gall-bladder. I carefully examined the liver but failed to find any secondary growths, and also was unable to detect any evidence of metastases in the abdominal organs or any thickening in the renal region on the left side. The right kidney was about half again as large as usual, but was perfectly normal considering the absence of the other. The urine, however, contained some pus and a little blood. On careful examination it was impossible to determine whether we were dealing with a cystic new growth or an abscess formation (Fig. 2). In the former case it would

have been difficult to tell why there had been no adhesions and why the surrounding liver substance was so firm. On the other hand, it would have been difficult to account for the elevation of temperature if no abscess was present. After some hesitation, we decided to remove the growth together with a small part of healthy liver substance. The gall-bladder was exposed, the cysitc duct clamped and cut, and the proximal portion of the duct tied off. The lower half of the liver was then readily drawn out through the abdominal incision. It was everywhere walled off from the abdominal contents by gauze, and a gauze rope was securely fastened around the liver about an inch above the point at which the cut was to be made. A cautery was called for, but, fortunately, as after-results showed, the instrument would not work. I then took a long blunt needle and started from left to right, passing mattress sutures through the entire substance of the liver. These were about 1 cm. apart, and each overlapped the other for about 2 or 3 mm. The greater number of sutures consisted of Pagenstecher thread, the supplementary ones being of catgut. After placing two or three mattress sutures I commenced to cut the tumor, at the same time being held taut. The sutures were then tied. This process of applying sutures and cutting and then tying was continued until the entire growth had been removed. Five or six large blood-vessels were encountered, which spurted freely. They were picked up with artery forceps and tied freehand with fine Pagenstecher thread. The entire growth was removed without the loss of more than two drams of blood. After the sutures had been tied there was absolutely no bleeding from the surface. After removal of the growth there remained a raw surface fully 16 cm. in length and 6 cm. in breadth. On making traction on the sutures I found that each of the raw surfaces could be rolled in upon itself, so that the two halves formed flaps. These were brought together until little or no raw substance remained. The ends of the sutures that had already been tied were utilized to bring the opposite sides

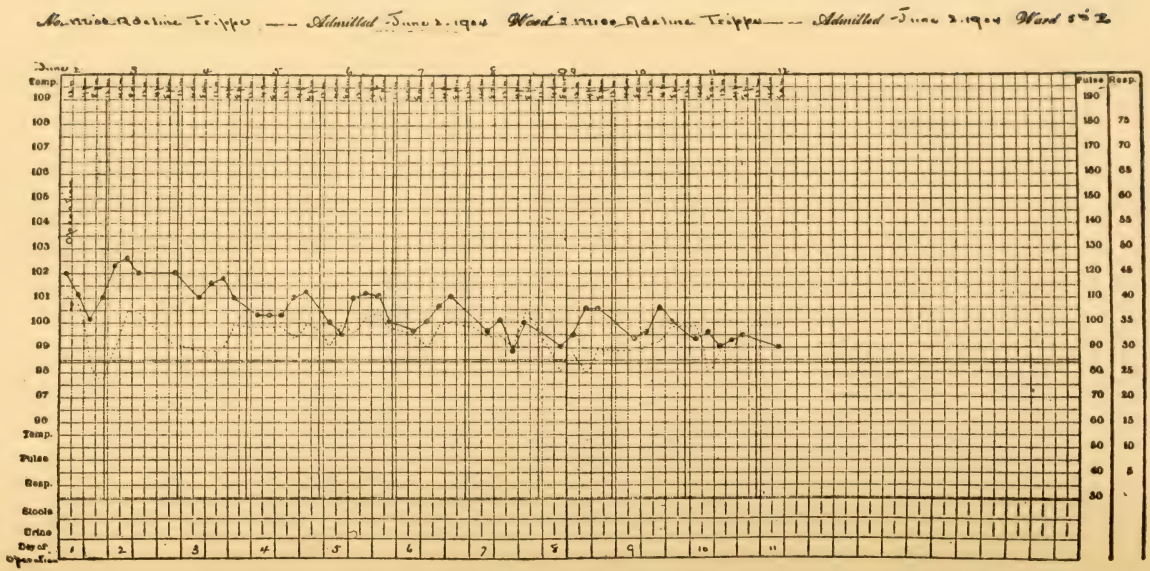


FIG. 3.—At the time of operation the temperature was 102°. It gradually dropped from the time of operation, reaching normal on the eleventh day. The pulse-curve follows the temperature-curve closely.

Fig. 1.—Secondary carcinoma of the liver. The liver is markedly displaced downward. Occupying the greater part of the right upper quadrant of the abdomen is a hepatic tumor. Downward, this reaches almost to the anterior superior spine; medianward, it passes nearly to the umbilicus; and laterally, it makes the abdominal wall prominent. The growth seems tense, is slightly lobulated, and, although sharply outlined, gradually blends with the liver substance. The edge of the liver forms a prominent ridge along the lower margin of the growth, and the gall-bladder is seen lying against the under and inner surface of the tumor. It is readily seen that when the cystic duct was severed the greater part of the liver could be rolled out of the abdomen. For a cross-section of the hepatic growth, see Fig. 2.

FIG. 2.—Secondary carcinoma of liver. As the specimen was very soft, it was impossible to cut it before hardening. Encircling the entire growth is a definite capsule, varying from 1 to 3 mm. in thickness. *a* is the sharp edge of liver substance that passes over the lower surface of the growth. This growth bears much resemblance to the parent kidney tumor, and in its central portion has undergone complete necrosis. The cut surface of the liver indicates the line of amputation. This growth measured 16 x 13 x 13 cm.

FIG. 4.—A large gall-bladder removed in its entirety. In the upper part of the figure is represented a cross-section of the cystic duct and of the surrounding tissues. The gall-bladder is greatly enlarged, uniformly distended, and resembles a sausage. The raw area seen in front corresponds to the attachment of the gall-bladder. At the top of the gall-bladder, and firmly attached to it, is a wedge of liver, and attached to the surface of this are several adhesions. For the appearance of the gall-bladder on section, see Fig. 6.

Fig. 6.—Longitudinal section of the gall-bladder. For the outer appearance, see Fig. 4. The cystic duct is slightly thickened. The walls of the gall-bladder are increased in thickness, and at point *a* reach a thickness of fully 1.5 cm. It is here that the picture is so suggestive of a commencing malignant growth that, without microscopic examination, it was impossible for us to say whether we were dealing with an inflammatory thickening or with a commencing cancer. Attached to the surface is a wedge of liver substance. The interior of the gall-bladder is much distended by two large and several smaller stones. There is a tendency toward partial partitioning off of the gall-bladder into smaller areas, as seen at *b*.

Fig. 7.—Complete removal of the gall-bladder, together with a wedge of the liver. The cystic duct is thickened. The gall-bladder is somewhat enlarged. Attached to it is a wedge of liver tissue, and at the upper portion we have a broad band of omental adhesions. For the appearance of the gall-bladder on section, see Fig. 9.

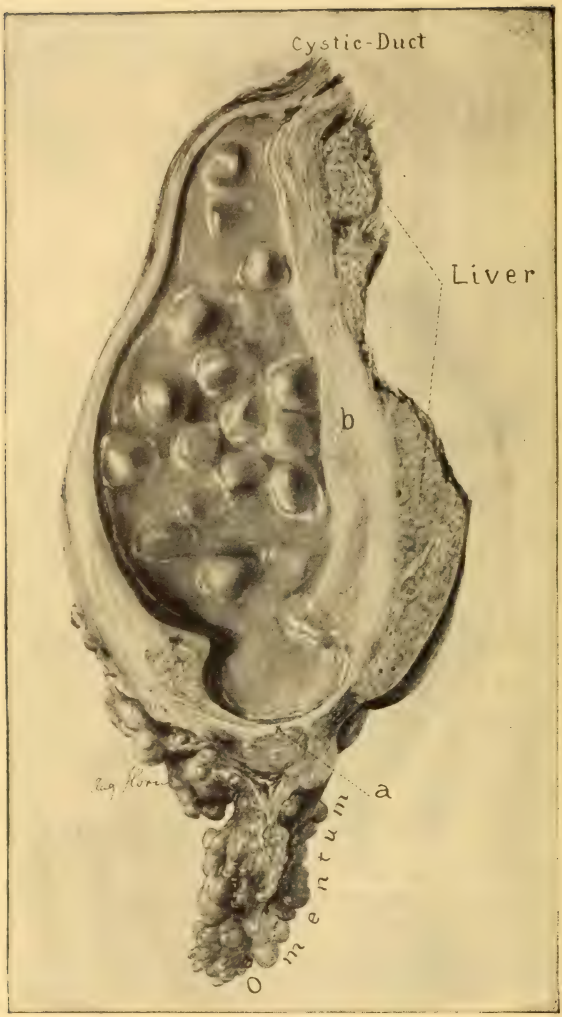


Fig. 9.—Longitudinal section of the gall-bladder, showing adherent liver substance and a cavity filled with pus and gallstones. The cystic duct is considerably thickened. The gall-bladder is enlarged. Attached to its surface at two points are masses of liver substance. The walls of the gall-bladder are much thickened, especially at *b*. At *a* there is atrophy, and had it not been for the adherent omentum, rupture of the gall-bladder at this place might very readily have occurred. The gall-bladder is filled with pus, and scattered throughout this are biliary calculi. See Fig. 7 for the unopened gall-bladder.

FIG. 10.—An enormous abdominal enlargement, due to an 89-pound cystic myoma. Sketch of the patient on the operating table. The picture is taken from the right. The general thickness of the abdomen is clearly seen. The tumor projects downward, extending almost to the knees. It is readily seen why the incision was nearly four feet in length, because it extended from the xiphoid cartilage to the symphysis, the tumor being removed in its entirety.

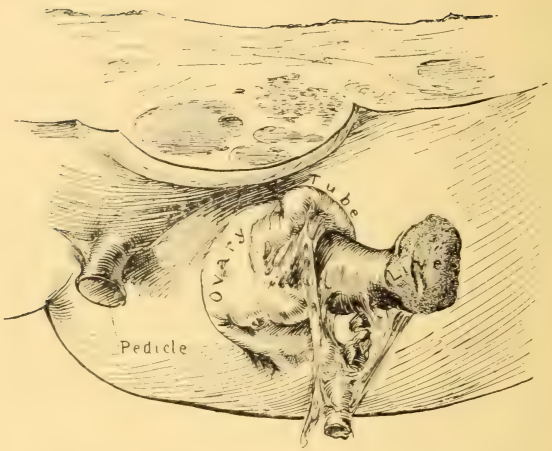


Fig. 11.—A small segment of the wall of the cystic myoma, showing a piece of liver attached to its surface. The cyst-walls in places reached 5 cm. in thickness. The pedicle of the tumor was very small. The tube and ovary were glued to each other by adhesions, and attached to them was a piece of liver substance which was so densely adherent to the appendages that it came away with the tumor.

together. Iodoform gauze was placed beneath the edge of the liver and also on its surface. Although the patient was apparently in a desperate condition when she went upon the table she stood the operation well, and when she was taken from the operating-room her pulse was only 104, and on the next day her temperature dropped to 100° F.; the general condition was excellent and the pulse 84. To look at her one would hardly realize that any operation had been performed. She made a rapid recovery and left the hospital in about four and a half weeks.

*November* 26, 1906. About August 4, 1904, suspicious areas of thickening were noted along the renal scar on the left side. In February, 1905, Dr. Trippe informed me that from time to time some sutures had come away through a small sinus, and that later the wound had healed entirely. At the present time there is marked local return of the cancerous growth where the kidney was situated, but there is not the slightest evidence of any return of the growth in the liver. There is no doubt whatever that the patient will soon succumb to the original renal growth, but it is now nearly four years since the kidney was removed, and the patient, up to a few weeks ago, had been able to go around everywhere.

*Hysteromyomectomy; excision of a large gall-bladder containing a thickened area suggestive of malignancy; resection of a wedge of liver substance with the gall-bladder (Figs.* 4, 5); *approximation of the cut surface of the liver by mattress sutures of catgut; recovery.*

Mrs. R., aged forty-seven years, seen in consultation with Dr. E. B. O'Reilly, of Hamilton, Ontario, on August 20, 1906. This patient for a number of years has had sugar in the urine at frequent intervals and has also had some albumin. Eight years ago she noticed a small lump in the uterus. This has greatly increased in size and has been associated with much uterine bleeding. At present the tumor is larger than an adult's head and extends above the umbilicus. The patient

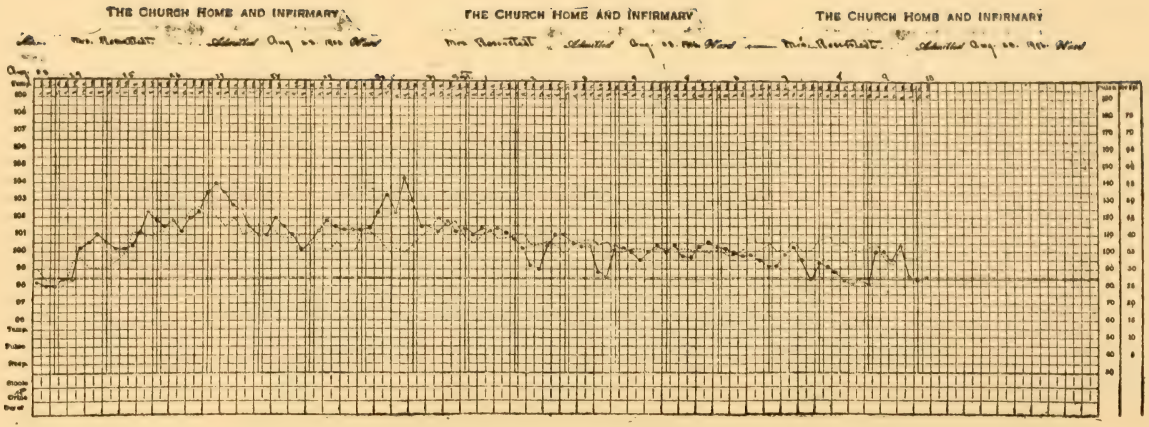


FIG. 5.—At the time of operation the temperature and pulse were normal. The temperature on the evening of the day after operation rose to 101°. The next day it rose to 102.4°, and on the fourth day reached 104°, came down considerably, but did not reach normal. On the seventh day it ran up to 104.4°. From that time it slowly dropped to normal. It will be noted that the pulse-curve was similar to that of the temperature.

is a very stout, well-nourished woman, weighing nearly 200 pounds.

*Operation.* On August 24, 1906, I operated in the Hamilton General Hospital, assisted by Drs. O'Reilly, Griffin, and Wallace. After removal of the large myomatous uterus, it was noticed that the gall-bladder was enlarged and prominent, and that it was filled with stones, and, furthermore, that projecting from the top of the gall-bladder there was a glistening area which appeared to be a new growth and to extend some distance into the liver substance. Fortunately, the liver was much farther down than usual and we were able to bring it fairly well out of the abdomen. We removed the gall-bladder and also a wedge of the liver (Fig. 4). We had no liver needles with us, nor did we have Hagedorn needles, which might have been introduced eye first. We accordingly used a bodkin, and mattress sutures of catgut were than introduced, which completely approximated the raw surfaces and checked all oozing. The patient stood the operation well.

*October* 5, 1906. I am just in receipt of the following communication from Dr. O'Reilly, who had charge of the pateint: "She is up daily and the wound is almost closed. There is no sugar; no albumin."

*November 12th.* I have just received the following communication from Dr. E. B. O'Reilly: "Patient leaving hospital tomorrow. Looks fifteen years younger. No sugar; no albumin."

*Description of the Gall-bladder.* Gyn. path. No. 10,419. This specimen consists of a large gall-bladder and a wedge of liver (Figs. 4, 6).

The gall-bladder is sausage-shaped, 11 cm. long and averaging 4 cm. in diameter. Attached to its upper surface are a few delicate adhesions. Where it comes in contact with the liver is a dense area 3 cm. in diameter. This, at operation, strongly suggested malignancy and had encroached markedly on the liver substance. The wedge of liver removed is 4 cm. in breadth and 5 cm. in length and varies from 1.5 cm. to

3.5 cm. in thickness. A small portion of the cystic duct is attached to the specimen and averages 5 mm. in diameter.

On section the gall-bladder walls are found to average 2 mm. in thickness. Near the cystic duct, however, there is a sacculation 1 cm. in length, 2.5 cm. in breadth (Fig. 6). Here the walls vary from 0.3 cm. to 1.5 cm. in thickness. The inner surface of the gall-bladder is slightly corrugated and has delicate trabeculæ. Near the cystic duct there is some pus. Almost completely filling the gall-bladder are two stones, one barrel-shaped and with a little excrescence on one side. This stone is 2.5 cm. in length, 2 cm. in breadth, and has facets on either end. Beside this is a large, irregular stone about 3 cm. in diameter. The gall-bladder also contains several smaller stones.

The thickened portion of the gall-bladder that was attached to the liver is 3 cm. in length and 4 cm. in breadth. It presents a pearly white appearance and has irregular yellow-mottled areas scattered throughout it. Even on section it is impossible to tell whether this is malignant or not. The relation between the thickened area and the liver is most intimate, and it would be impossible to separate the gall-bladder from the liver by blunt dissection without injury to the liver.

*Microscopic Description.* Sections from the inner wall of the gall-bladder in many places show a typical granulation tissue, consisting of new connective-tissue cells, capillaries, and of polymorphonuclear leukocytes. In the vicinity of such areas no epithelial lining is present, but at protected points the epithelium is cuboidal or high cylindrical. Even at such points, however, the underlying stroma shows marked small round-celled infiltration.

The great thickening noted in the upper end of the gall-bladder is due in part to massive infiltration by small round cells, but also in a large measure to the replacement of adipose tissue by connective tissue. The connective tissue of the gall-

bladder merges imperceptibly with that of the liver capsule, and in the liver we have quantities of small round cells surrounding the lesser bile-ducts. Whether the gall-stones antedated the inflammation it is impossible, of course, to determine.

*Diagnosis.* Chronic purulent cholecystitis, with extension of the inflammatory process to the liver.

*Obscure thickening of the gall-bladder, with dense adhesions to the liver and omentum; cholecystectomy, with removal of a wedge of liver* 5 *x* 5 *cm. (Figs.* 7, 9*); approximation of raw liver surfaces by mattress sutures of catgut; recovery.*

F. G., aged thirty-two years, married, white; referred to me by Dr. Stump, of Philippi, West Virginia. She was admitted to the Church Home, October 12, 1906. She complained of sudden, cramp-like pains in the epigastrium. Apart from the fact that one brother had died of "scrofula" about eighteen years before, her family history was good. Her menstrual history had been of no importance. She had been married fourteen years and had had six children. Five weeks previously she had complained of sudden, cramp-like pains in the epigastrium. These occurred intermittently for the first twenty-four hours, and the pain became so severe that she had to go to bed. She was nauseated, took several emetics, but vomiting gave only temporary relief. She remained in bed for two weeks. On the morning after the attack commenced the patient noticed a swelling beneath the right costal margin. This caused much discomfort. By the end of the first week the pain had ceased completely, but since the first attack there had been occasional sharp pains. On admission the tumor was smaller than when first observed.

*Examination.* The tumor is situated in the right upper abdominal quadrant. It can be moved around to some extent. It feels in contour very much like a prolapsed right kidney; it is firm on palpation. The patient gives an indefinite history of jaundice five weeks ago, otherwise we should have felt relatively certain that we were dealing with a kidney.

The patient has not been able to work since her attack five weeks ago. She thinks she has lost in weight.

*October* 14, 1906. A careful cystoscopic examination showed perfectly normal urine and a normal bladder mucosa. On examination the blood showed a leukocytosis of 14,000, although the temperature and pulse, on her admission, were normal.

*Operation.* October 15, 1906. On opening through the right rectus we encountered a thickened and adherent omentum. This was fast to the gall-bladder, and the stomach and transverse colon were also firmly adherent to it. The gall-bladder seemed to be exceedingly firm and was encroaching upon the liver to some extent. It was necessary to free the gall-bladder from the liver, but the connection between the two was so intimate that we decided to take a wedge 5 cm. in breadth out of the liver (Fig. 7). The raw areas in the liver were brought together by eight mattress sutures of catgut. The gall-bladder was now freed down to a point where the cystic duct was not much thickened. It was tied off on both sides and the duct cut across with a cautery. After all bleeding had been checked, three iodoform drains were carried down to the stump of the cystic duct and to the under surface of the liver. The bleeding was slight and was reduced to a minimum.

In this case we were somewhat suspicious, from the thickening of the gall-bladder, from the densely adherent omentum, and the intimate relation between the liver and the gall-bladder, that a malignant growth was commencing, and consequently decided that it would be much safer to at once remove a wedge from the liver.

The patient was returned from the operating-room in good condition, and spent the remainder of this day fairly comfortably; she vomited once. On the second day she vomited 540 c.c. The pulse was about 130; the temperature dropped from 101° to 99° in the evening. On the second day there was considerable vomiting and bile-stained fluid was present

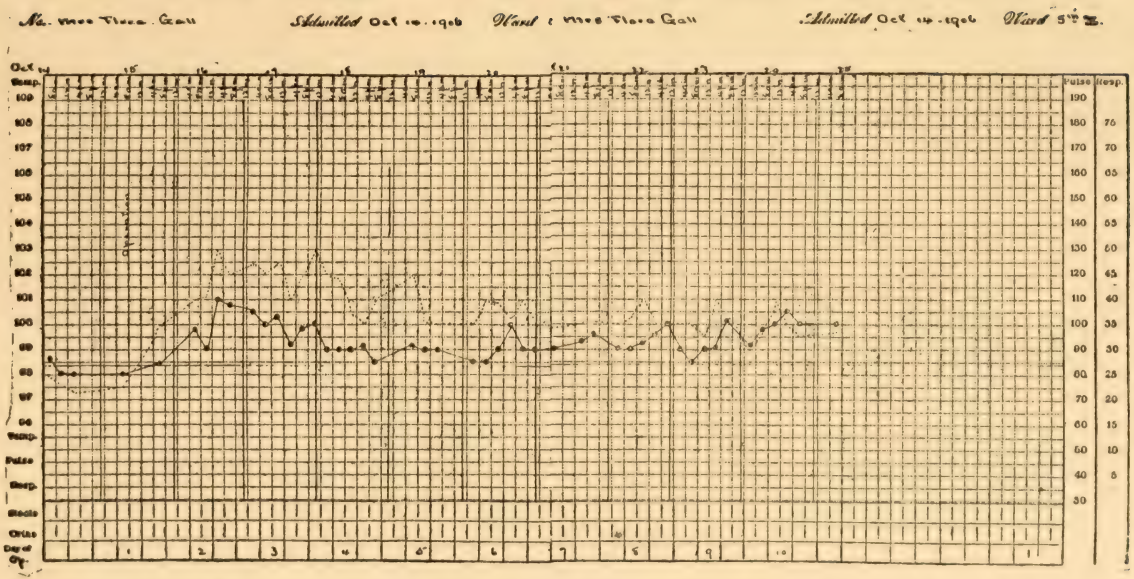


FIG. 8.—As will be noted, at the time of operation the temperature was normal. On the next evening it rose to 101°, and then gradually dropped to normal, only once subsequently reaching 100.5°. The pulse at operation was normal. The next day it reached 130, and remained between 120 and 130 until the end of the third day, when it gradually dropped.

in large amounts on the dressings. This evidently came from the cut liver. Dressings changed on the second day were saturated. On the third day there was slight vomiting and the pulse was irregular. From this time on the patient made a very satisfactory recovery.

*November* 14, 1906. The patient's wound has healed, and she is to go home on the 17th.

*Description of Gall-bladder.* Gyn. path. No. 10,418. This specimen consists of the gall-bladder, a wedge of the liver, and of a densely adherent piece of omentum. The description and measurements are of the hardened specimen.

The gall-bladder itself is 9 cm. in length and is pyriform in shape (Fig. 7), varying from 1.3 to 5 cm. in diameter. The cystic duct is 8 mm. in diameter and considerably thickened. Intimately attached to the gall-bladder is a wedge of liver, 4 cm. in breadth and 4 cm. in length. This wedge of liver varies from 2 mm. to 2 cm. in thickness.

The omentum is snugly tucked down to the edge of the liver, and forms an almost integral part of the free surface of the gall-bladder for a distance of 5 cm.

On section, the gall-bladder wall is found to vary from 3 to 6 mm. in thickness. The inner surface presents a worm-eaten appearance, as if it were the wall of an abscess. At the fundus is a marked softening, the wall has given way over an area 1.2 cm. in diameter, and at this point the outer covering is only 1 mm. in thickness. Here it is that the omentum is most densely adherent (Fig. 9).

The gall-bladder is filled with pus, and scattered throughout the cavity are many yellow-faceted gallstones which average 5 mm. in diameter. The relationship between the gall-bladder and the wedge of liver removed is intimate. The connective tissue of the gall-bladder wall can be seen penetrating the liver. It would have been impossible to separate the gall-bladder from the liver without a great deal of tearing. The danger of infection would then have been infinitely greater.

Cover-slips from the pus show quantities of polymorphonuclear leukocytes with detritus, and here and there long and narrow bacilli. No culture was made, as we wished to harden the specimen before examining.

Sections from the gall-bladder show that in some places the lining epithelium, consisting of high cylindrical cells, is intact, and that the gland-like spaces, due to depressions, present a characteristic appearance. The stroma of the folds at such points shows small round-celled infiltration and here and there polymorphonuclear leukocytes. Polymorphonuclear leukocytes are also present between the individual epithelial cells, and the cavity of the gall-bladder contains desquamated epithelium, small round cells, leukocytes, and detritus. At other points the lining epithelium has entirely disappeared and we have nothing but typical granulation tissue, consisting of young connective-tissue cells and capillaries, with here and there on the surface numerous polymorphonuclear leukocytes, and in the deeper portions cells that have taken up the brown pigment, evidently from hemorrhage. At one point in this granulation tissue we have a typical giant cell containing at least 25 or 30 small round vesicular nuclei bunched chiefly in the centre. There is, however, no suggestion of tuberculosis. The underlying stroma shows a good deal of small round-celled infiltration, and in the walls are abscesses. The inflammatory changes persist throughout the walls of the gall-bladder. The adherent omentum has, to a great extent, undergone fibrous change; in some places there is little more than old connective tissue, with here and there foci with small round-celled infiltration. Clumps of fat globules—remnants of omental tissue—still persist throughout this fibrous tissue. The inflammatory thickening extends outward until it reaches the liver substance, and it is impossible to tell where this fibrous tissue stops and the capsule of the liver commences. The nuclei of the liver cells vary considerably in size and in their staining qualities. We have a good deal of small round-celled infiltration and numerous eosino-

philes. The eosinophiles are also abundant in the thickened wall of the gall-bladder.

*Diagnosis.* Acute and subacute cholocystitis, with large quantities of pus and gallstones in the gall-bladder; implication of the adjoining liver tissue in the inflammatory process; dense omental adhesions undergoing fibrous change.

Removal of a wedge of liver substance with the gall-bladder certainly has a somewhat restricted field. Personally, in the majority of cases, I would prefer to drain the gall-bladder merely, agreeing with those who consider the thorough drainage of the dammed-up bile as only secondary in importance to removal of the stones so frequently found.

In some cases it is impossible to remove the gall-bladder without tearing the liver to some extent, and in a limited number of cases it is impossible to exclude the possibility of carcinoma. In all such cases I think it just as well to be on the safe side, and to remove not only the gall-bladder, but also a wedge of the surrounding liver substance. We have found one disadvantage in the entire removal of the gall-bladder; that is, the sudden elevation—in some cases—of temperature after operation. In both of our cases there was, as will be seen from the charts, an elevation of temperature, but it gradually dropped to normal. In our case of resection of a portion of the liver for tumor, however, it will be noted that the temperature, which was over 102° at the time of operation, gradually dropped to normal.

### SUTURE OF ACCIDENTAL TEARS OF THE LIVER.

I have had two cases of this character. In the first there had been an abdominal abscess which had communicated with the liver, and in exploring the cavity I tore the liver substance. The bleeding was excessive, but was rapidly and completely controlled by four mattress sutures of catgut. In the second case, in removing an 89-pound subperitoneal

pedunculated myoma, a piece of liver substance was found adherent to the upper surface of the tumor. I was particularly anxious to close the abdomen without drainage, and by passing Hagedorn needles backward the bleeding was at once checked by a few mattress sutures of catgut.

*Abscess between the abdominal peritoneum and omentum four weeks after labor; implication of the liver, the edge of the liver containing an abscess; secondary exploration in the region of the liver, with marked tearing of the friable organ; hemorrhage controlled with blunt liver-needles; complete recovery.*[1]

Gyn. No. 12,456. A. M., seen in consultation, October 20, 1905, and at once admitted to the Johns Hopkins Hospital. She is thirty-six years of age, married, white. Her family and previous history are negative. She complains of a good deal of abdominal discomfort and of fever. The patient has been married seventeen years and has had ten children; the eldest is fourteen years of age; the youngest was born September 19, 1905, just one month before her admission. Her labors have all been difficult and instrumental, and on several occasions there was tearing of the perineum. Four years ago the perineum was repaired at the Johns Hopkins Hospital. For several days prior to her labor in September she had been in bed with a fever. Her delivery was instrumental and was followed by a most profuse hemorrhage. Fever and chills continued for four days after delivery, and since that date she has had chills and fever, and marked pallor has been noted. On admission to the hospital she was exceedingly anemic, with the hemoglobin 26 per cent. and the leukocytes 11,200. There were distinct hemic murmurs, but the heart was normal. The lung sounds were also normal. The right eye was inflamed; a white scar was visible on the cornea and a distinct purulent exudate was made out.

The abdomen was distended, and just to the left and above the umbilicus was a definite area of bulging. Just to the left

[1] Abstract from the Johns Hopkins Hospital Bulletin, 1906, p. 153.

of the umbilicus, and extending outward, was an indurated area 2.5 cm. in diameter. This was somewhat pointed. The lower part of the abdomen was hard and suggested a large abdominal tumor; there was no reddening. There was more or less tenderness over the entire abdomen, especially over the small tumor or elevation in the left side. On pelvic examination nothing could be detected. The cervix was high up, the uterus was freely movable, and the appendages were perfectly free.

*Operation.* October 21, 1905. An incision was made over the hardened indurated area on the left side. On opening the abdomen we encountered a large abscess containing foul-smelling pus. On careful exploration of the cavity I found that it continued upward almost to the free margin of the ribs and downward to within 8 cm. of the pubes. It apparently lay between the anterior abdominal wall and the omentum. No intestines or abdominal contents were to be seen. The cavity was loosely packed with iodoform gauze, and the patient left the table in a precarious condition. Cultures from the pus showed no organism, but anaërobic cultures were not made. After a few days large quantities of pus were discharged. The temperature kept up; the patient became delirious and had to be isolated.

On November 3, 1905, the hemoglobin was 25 per cent. and the leukocytes were 15,200. On the following day the patient was brought down to the operating-room, as her condition was not satisfactory. We could detect an area of induration along the free margin of the ribs on the right side. Artery forceps were introduced into the opening and carried out to where the induration had been detected on the right side. We then cut down upon the indurated area in the flank, an incision being made on the outer side of the rectus. The peritoneum was thickened and there was much edema in the peritoneal tissues. The liver was found lying just beneath the incision, and over a considerable area it appeared to be necrotic. It broke up very readily under the examining finger,

and there was free hemorrhage. The insertion of three or four catgut sutures, however, with a blunt needle checked the oozing completely. We cut down on the edge of the liver and exposed a large pus cavity.

Apparently the abdominal abscess had had its origin in the gall-bladder or had been a primary liver abscess, but in no way had implicated the pelvic organs, and had apparently been almost entirely anterior to the omentum. The patient gradually improved.

*November* 20, 1905. She was taken out of doors, although her condition was still precarious. From this time on her mental condition rapidly cleared up, and a decided improvement was noted each day.

She was discharged on December 21, 1905, with the abdominal wound completely healed. There was no tenderness at any point. The hemoglobin was 76 per cent., and the mental condition was perfectly satisfactory.

In this case it was remarkable to see with what ease we could control the liver-oozing. It was only necessary to introduce a few catgut sutures through the friable material and the bleeding was absolutely stopped. Had we used too much force, however, in tying, the sutures would have undoubtedly cut through the liver tissue.

*Successful removal of an 89-pound cystic myoma intact (Fig. 10); piece of liver 3 cm. in diameter removed with tumor (Fig. 11); suture of liver; recovery.*

Mrs. McA., seen in consultation with Dr. Marshall Smith on June 25, 1906. She is fifty-eight years of age. Twenty years ago she noticed a tumor in the lower abdomen, and was later seen in consultation by Dr. William T. Howard, of the University of Maryland, who advised operation. The tumor gave her little inconvenience until three or four weeks before her admission to the Church Home and Infirmary, which she entered on July 27, 1906.

*Operation.* July 30, 1906. There was very marked abdominal distention. Prior to receiving the anesthetic the

patient was thoroughly washed up and all preparations were made for the operation, so that she might remain as short a time as possible under ether. It was impossible for her to lie down; consequently she was operated upon in the sitting posture. The growth was removed with a great deal of difficulty, as it was everywhere adherent to the anterior abdominal wall and to the omentum. We found large vessels going to the tumor from the omentum, also numerous vessels from the stomach and transverse colon, some of them reaching fully 7 or 8 mm. in diameter. The adhesions were gradually shelled off by my hand, but I had to use the utmost care and do all liberating under sight, as the rupture of one large vein might have caused the loss of the patient's life before the hemorrhage could be checked. The pedicle of the tumor was not more than 1.5 mm. in diameter, and was situated directly beneath the ribs.

On removal of the tumor I found that I had brought away a piece of liver substance 3 cm. in diameter. We had no liver needles handy so I used a sharp Hagedorn needle, running the eye through instead of the sharp end. This device answered admirably, and the bleeding was easily checked by four catgut sutures. The uterus and the right tube and ovary lay under the surface of the liver. The abdomen was closed without drainage, and the patient made a very satisfactory recovery. In this case, of course, the removal of this huge myoma, which, as far as we can learn, is the largest myoma successfully removed, was the most important feature, and the bringing away of the piece of liver a minor consideration. The bleeding in the liver was checked with the utmost ease, in fact much more rapidly than we are usually able to control bleeding from the uterus; and, furthermore, it was checked so thoroughly that we did not deem it necessary to drain the abdomen.

Liver surgery is still a vaguely explored field, and will certainly in the near future engross much of the attention of

the abdominal surgeon. We thoroughly agree with the *dictum* of Mikulicz, expressed in the terse sentence: "To my mind Kousnietzoff's needle is the egg of Columbus in the technique of liver surgery," and with W. W. Keen when he says: "After my experience with these three cases, I should hardly hesitate to attack almost any hepatic tumor without regard to its size."

## APPENDIX.

While operating on my first case I accidentally discovered the secret of applying liver sutures. An assistant gave me a long, straight needle that had lost its point, and as we did not wish to delay, I used this, and was surprised to see the absolute lack of hemorrhage from the needle holes. Several weeks later, when going over the literature, I found an article by Kousnietzoff, and read the hearty commendation of his work by Mikulicz. Later, I had needles made. These, instead of being wedge-shaped on cross-section, as were those of Kousnietzoff, are almost flat, and further, they are much more slender, making a smaller hole in the liver substance. The modifications are, however, so slight that these needles are merely improved Kousnietzoff needles, and all the credit for introducing the needle that will push the vessels to one side or the other instead of piercing them belongs to Kousnietzoff.

I sent long and curved liver needles to some of my friends in different parts of the country. From one of them, Dr. M. H. Everett, of Lincoln, Nebraska, I have recently heard, and his letter is of much interest, as he has had the same satisfaction with this method of suturing the liver as we have had:

"In answer to your recent query concerning our use of the needles you so kindly gave us, I am glad to say that they have proved of great value. They are so simple and so efficient and easy in handling, that for us liver surgery has lost much of its fear. We have had only the opportunity to apply them in three operations of any magnitude. The first operation was a resection for a carcinoma of the gall-bladder, together with a portion of the liver 7 x 5 cm. in extent.

"In the second, a somewhat smaller portion was removed on account

of severe hemorrhage from the walls of an abscess at the upper pole of the gall-bladder.

"In the third case, a portion of liver in a case of multiple gummata was removed for diagnosis.

"In all of the above the suturing was rapid and sure with these needles, and controlled the hemorrhage perfectly. Besides these applications, the needles have proved of value in many instances where the liver substance had been injured by the separating of a gall-bladder or by the breaking up of strong adhesions.

M. H. EVERETT."

Fig. 1.—Gonorrheal exostosis of the os calcis.

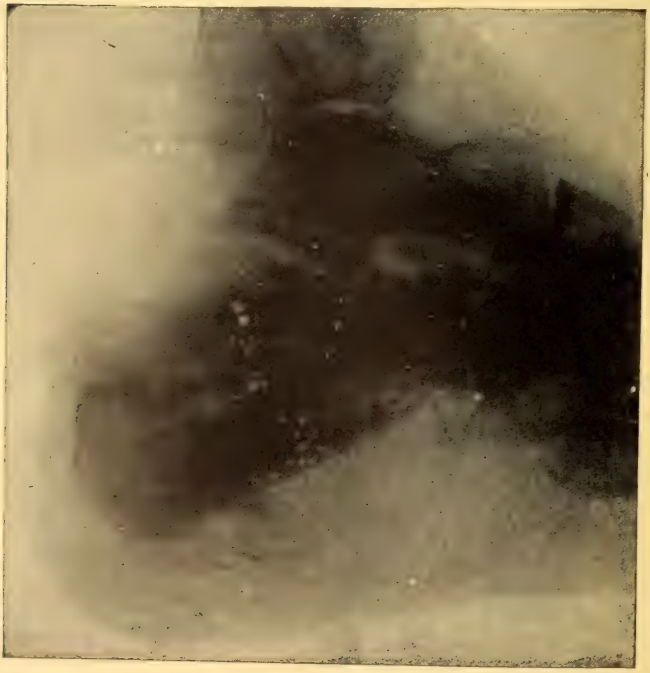

Fig. 2.—Gonorrheal exostosis of the os calcis.

# RESULTS OF OPERATIONS OF EXCISION OF GONORRHEAL EXOSTOSES OF THE OS CALCIS.

By William S. Baer, M.D.,
*Baltimore.*

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I have here an interesting case of what we may term painful heel. This condition may occur from different causes. In this case the pain is referred to the plantar surface of the os calcis. We have had 10 such cases, and of this number 8 have been operated on. The etiological factor is what interests us. In the 8 cases which have been operated on we have been able to grow pure cultures of the gonococcus from these heels in 3 cases. In 4 others the organisms have been found in the plantar fascia and muscle attached to the heel.

The history of the cases is generally that of an acute urethritis, and following it there is a painful condition in the heel. It is generally bilateral, although in some cases it will be unilateral. The pain is characteristic. It is referred to a definite point—that is, to the tubercle of the os calcis on the plantar surface of the os calcis. Movements in all directions are normal. Occasionally, however, there is a certain amount of thickness about the lateral surface of the os calcis.

If the pain is severe, as in this case, the patient does not walk on the foot; he relieves the pain by taking the weight from the heel and walking on the ball of the foot. Sometimes the pain is so great that the patient is incapacitated for walking. The localized painful area is about 1 cm. in diameter. This boy was operated on seven days ago. He still feels a painful spot, although the exostosis has been

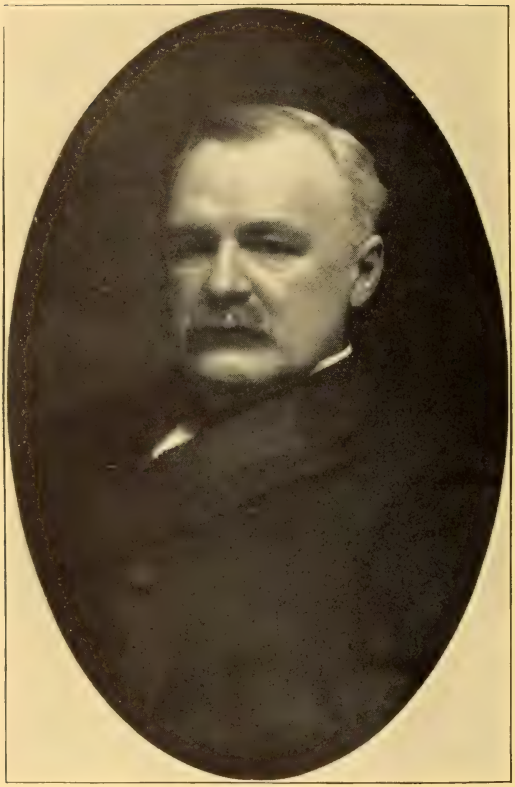
removed. If I press on this sensitive area it causes him considerable pain.

I will pass around one or two pictures showing various exostoses of the os calcis. In order to get these patients well, it is very essential to remove the exostoses entirely.

We have other conditions of the heel from which it is essential to make a differentiation, as, for instance, so-called osteoarthritis in heels, or policeman's heel. In these cases of policemen's heel, operative measures are sometimes contraindicated, as the results are not so good. The differentiation should and can be made between osteo-arthritis of the heel and infectious arthritis from gonorrhea by the character of the exostosis. Gonorrheal infection seems, in a great measure, to be limited to the portion of the bone on the plantar surface of the os calcis.

The operative treatment consists in making an incision on the side of the heel, going down above to the plantar fascia, removing a small portion of bone through such an incision. Of the eight cases which we operated on (not counting this one), the pain in the heel has been relieved in all of them. Some of these have now gone on for three or four years without any recurrence of the trouble.

Here, in brief, we have a disease whose principal symptom is pain in the heel, coming on from three to nine months after a urethritis. There are no acute local symptoms, pain gradually increases, generally involving both heels, and due to a small spicule of bone which, when removed, causes perfect relief.



LEHMAN H. DUNNING.

# IN MEMORIAM.

## LEHMAN H. DUNNING, M.D.

(From the memorial addresses before the American Association of Obstetricians and Gynecologists, by Hugo O. Pantzer, M.D.)

On January 4, 1906, death took away one of our most distinguished fellows in the person of Dr. Ledman H. Dunning. An eminently active and useful life, rich in high aim and great accomplishments, came to an untimely end. A kindly nature of strong moral quality; an enthusiastic and tireless worker; a serious, intent, and forceful presence on all occasions.

Dr. Dunning was born at Edwardsburgh, Michigan, April 12, 1850, the son of a successful farmer. His ancestors were originally English, and had settled in the State of New York. His grandfather, Dr. Isaac D. Dunning, was a leading practitioner in medicine at Aurora, New York, for thirty years, and then emigrated to Michigan about 1836.

Our departed fellow was educated at the Edwardsburgh High School. Contrary to the will and without the support of his father, he selected the profession of medicine for his life's work. He attended two courses of lectures at the Medical Department of the University of Buffalo, and completed his studies at Rush Medical College, Chicago, where he graduated in January, 1872, with special honor. Dr. Dunning began the practice of medicine at Troy, Michigan; removed to South Bend, Indiana, in 1878, and to Indianapolis in 1889, where he remained to the end.

At Troy his earnest and energetic application and wide-awake interest brought him conspicuous and varied recognition. His desire for literary improvement soon made him a leading factor in the lyceum work of his little town and vicinity. For a time he was District Superintendent of Public Instruction. He was appointed correspondent of the Michigan State Board of Health. Performing the duties of this office he acquired his

first experience as a writer on medical subjects. Dr. Dunning was a devout and practical Methodist all his life. At this early stage we find him in the position of Superintendent of the Sunday School of the Troy Methodist Church. Meanwhile he had worked into a large medical practice. This was of the severe country type, which appeased well his ardent desire for work. In 1875, he was married to Miss Harriet Beauchamp, of Edwardsburgh. Mrs. Dunning and three children, namely, Miss Florence, Lehman, Jr., and Herbert survive him; the elder son is a student at the Indiana Medical College.

At South Bend he sought and found a larger field for his ambition. Modern surgery and gynecology were then just in their early dawn. A nature like his, inherently drawn to deeds, intuitively found these fields. With avidity and zeal he applied himself to their study and practice. The current literature he gleaned with eagerness, and practical demonstration was sought at post-graduate courses. His contributions to medical literature during this time give fair index of his doings. It appears that he gave much and successful attention to genito-urinary diseases. He was one of the first in his part of the country to treat the floating kidney by fixation; and he also did nephrectomy for suppurative disease. His early gynecological papers are case reports with remarks, evincing his thorough study and intense interest in the subject. They pertain to development deficiencies and anomalies of the uterus; to pelvic peritonitis; to mammary and uterine cancer. These papers were read at county and Indiana State society meetings, and were published in various journals in different parts of the United States. They served well to give him renown. Accordingly, in 1889, he was appointed Adjunct Professor of Diseases of Women in the Indiana Medical College. Preparatory to his removal to Indianapolis he took an extensive trip abroad, visiting the hospitals of Vienna, London, and Paris. Upon the death, in December, 1899, of the illustrious pioneer in gynecology, Dr. Thomas B. Harvey, who had held the Chair of Medical and Surgical Diseases of Women since the organization of the Indiana Medical College in 1869, Dr. Dunning was appointed his successor. With this advent came to him the opportunity to fully unfold his truly phenomenal capacity and capability for work. During the declining years of Dr. Thomas B. Harvey, the former had quite undisputed and effectual sway. Dr. Joseph Eastman, the famous Professor of Gynecology of the Central College of Physicians

and Surgeons of Indianapolis, at that time was in his zenith. Notwithstanding, Dr. Dunning as the sole representative in gynecology of the Indiana Medical College, early in his Indianapolis career, enjoyed the practically undivided patronage of a large alumni scattered all over the State of Indiana. This afforded him a task in and out of the city which alone would have exhausted the capacity of any ordinary man. Inexperienced in clinical and didactic lecturing, his serious and untiring devotion soon made him a fluent, clear, and terse lecturer. Besides didactic lecturing to senior and junior classes and giving two or more clinics a week at the City Hospital and City and College Dispensary and at the St. Vincent's Hospital, he lectured on gynecology to the pupil nurses of four training schools.

Practically unacquainted with the more strictly scientific and laboratory methods of modern medicine, he zealously applied himself in his odd hours to the study of microscopic pathology. In this field he was destined, by his own discovery, to successfully refute the hitherto unchallenged view that endometritis in the senile was ever of acute origin; he having proven the opposite to be true. He always held important positions on various committees connected with college, medical society, and hospital work, and never failed to give them prompt and conscientious consideration. He wrote many medical papers and addresses to medical and nurses' associations. All evince his earnest spirit and comprehensive knowledge. There is probably no moot question of his day in his specialty upon which Dr. Dunning did not give expression to his views on one occasion or other.

Many honors have come to Dr. Dunning. Especially noteworthy are his election to the office of President of the Indianapolis Medical Society, the Indianapolis Gynecological Society, the American Association of Obstetricians and Gynecologists, and last, 1905, to the office of Chairman of the Section of Gynecology of the American Medical Association. He was elected to Fellowship in the Southern Surgical and Gynecological Association in 1900.

Away from the common tendency to materialism among scientific students, Dr. Dunning all his life was "ever conscious of the spiritual relations of nature and religion." As previously mentioned, he was a devout and dutiful member of the Methodist Episcopal Church, holding important offices in it at all stages, and giving freely of his time. He presented numerous

papers and addresses to various church gatherings of local and national character, including Young Men's and Women's Christian Associations. His work along these lines found great recognition and praise in the resolutions adopted upon his death by the Board of the Central Avenue Methodist Episcopal Church, of which he had been for many years, and at the time of his death, an active and highly esteemed member.

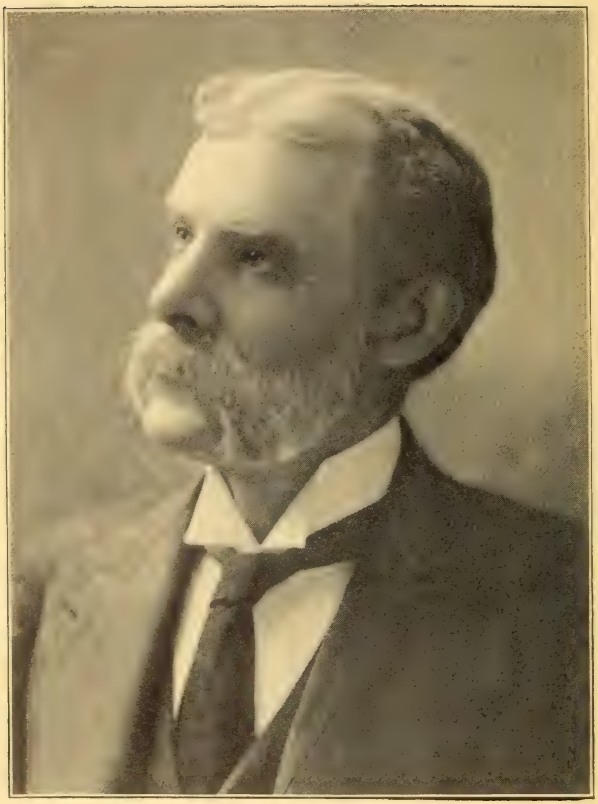
It seems natural, though hardly feasible, that besides all that has been mentioned, Dr. Dunning should have given additional time and thought to special literary studies. While travelling on trains to and from his professional calls, he invariably carried a pocket edition of some classical author with him. His vintage in this field shows a notable yield, in the papers he furnished to the Indianapolis Literary Club. The subjects of his papers show his great interest in Shakespeare. For awhile he conducted the work of a small class composed of advanced Shakespeare students of the Club.

Dr. Dunning was essentially gentle and kind, easily stirred to the depths of his nature, and aroused to deeds of active kindness. He was a loving husband and a prideful father, whose yearning nature, in the midst of his busy career, was yet able to extract from his family relations the sweetest joys of life. Mrs. Dunning has been an invalid for many years and his loving and duteous devotion to her constitutes one of the great chapters in his life. The most solicitous thoughts of his last day were given to the consideration of a change of climate for his wife. In the toil and din of his everyday life his emotional nature rarely found opportunity for expression.

Largely by Dr. Dunning's efforts a gynecological society composed of all the gynecologists in the Indianapolis field was organized, which met once a month during the last three years of his life. While Dr. Dunning seemed unaware of and in his actions little perceptibly displayed the effects of the drain and strain upon his constitution, the critical eye could not but in his late years observe the mark of his rapid aging. His end came with suddenness. After an ordinary day of work and worry, he had enjoyed a hearty meal and spent a quiet evening in unusual felicity at his home. He retired at 10 P.M. He was awakened at past midnight by an agonizing pain in his left chest. This returned at short intervals, but seemed definitely subdued under an opiate at about 2 A.M. At his urgent request his medical attendant left him. He had expressed his belief that all trouble had subsided, and associated its occur-

rence with the heavy evening meal. He was found dead the next morning, in seemingly calm and peaceful repose. Death had supervened without further struggle. He had died from angina pectoris, a doctor's disease.

To his surviving friends it must ever be a regretful thought that Dr. Dunning could not have had the quiet philosophic years of advanced age; the golden fruitage of a great experience.



JOHN CLARK Le GRANDE.

## JOHN CLARK LE GRANDE, M.D.

Dr. Le Grande was born in Calhoun County, Alabama, December 6, 1854; died March 21, 1906, in Birmingham, Alabama. Between these two dates is embraced the life of a man of tireless activity.

Dr. Le Grande received a common school education, and graduated in medicine from the Southern Medical College in Atlanta, in 1880. At that time the Southern Medical Colleges did not afford very superior advantages, so that he lacked the early professional training which is so essential to success in the medical profession. Notwithstanding these disadvantages, by the force of energy and perseverance he surmounted the many difficulties which confronted him in the beginning of his professional career, and in a few years achieved a prominent place in his profession.

In his life he exemplified what can be achieved by the self-made man. He assisted in organizing the Calhoun County Medical Society, and was the Secretary for a number of years and later was elected President. He early became an active and enthusiastic member and supporter of the Alabama State Medical Association, and was elected a Counsellor at Mobile in 1889, and in 1899 was elected President of the Association, presiding over the meeting held in Montgomery in 1900. In addition to these honors he was elected Vice-President of the American Medical Association at the meeting in Baltimore in 1895, and presided in Atlanta over the meeting of the Association during the greater part of the session held in June, 1896. In 1901 he was elected President of the Tri-State Medical Society of Tennessee, Georgia and Alabama, at Nashville, Tenn., and presided at the meeting in Birmingham in 1902.

In all of these high positions of honor and trust he was faithful, loyal, and true to the organization which he represented. He was a firm believer in medical organization and did much to further the best interests of his profession.

Dr. Le Grande became a member of the Southern Surgical and Gynecological Association in 1899, and was a faithful attendant.

He was, perhaps, better known in connection with medical journalism, having founded and conducted successfully the *Alabama Medical Journal* for the past eighteen years. He was an untiring worker in this field and was foremost in every movement for the elevation of his profession and the good of humanity.

In 1902 he became actively connected with the Birmingham Medical College, having been Secretary and Treasurer, and Professor of Obstetrics. Both of these positions he filled with credit to himself, and as Secretary of the College had charge of its business affairs, and was largely instrumental in shaping and directing its policy, to which he devoted much time and attention.

If we stop to ask the ultimate reason why all these honors crowned a comparatively short career, the best answer that can be given is that he was at all times and at all places absolutely loyal to his profession. He translated the highest conception of ethics into his daily life; the way was naturally plain to him and he rigidly followed it, because he knew it was right and because all devious ways were foreign to his every thought.

The role Dr. Le Grande performed should be a great comfort to his family and an inspiration to every poor, struggling, medical student. He began medicine without that preparation which a good education affords; and his early career was one of great hardships, struggling toil, and constant privations. Dr. Le Grande had more than the ordinary success which comes to a physician.

He removed to Anniston, early in his professional career, that he might have better opportunities for work and a wider field for usefulness. For the same reason he later moved to Birmingham. He was in the fore-rank of every cause which looked to the elevation of his profession and the good of humanity. Honest to a fault he could always be placed on any question. He was always on one or the other side of every question, and when he took a stand on any cause he became a champion of vigor, who feared no foe, and would run counter to friends to follow his convictions.

When Dr. Le Grande founded the *Alabama Medical Journal* he took upon himself a responsibility that few men would voluntarily shoulder. The lamented Cochran once said that he hoped some progressive man with brains and courage would publish a medical journal in the State, as the job was too big

an undertaking for him. We all know how well Dr. Le Grande succeeded! He went into it welcoming infinite details and unknown responsibilities, and met every obligation promptly, cheerfully, and successfully. Nothing else he ever undertook gave such a paradigm of his ambitious determination to do something, and be something in the progress and advancement of his profession, and it may be said that his work was well and nobly done.

## BENJAMIN WALTER TAYLOR.

Benjamin Walter Taylor was born at Edgehill, near Columbia, S. C., February 28, 1834, and was a representative of one of the oldest families of this section of the State. He died at Columbia, December 27, 1906, aged seventy-one years, from pneumonia. The primary education of Dr. Taylor was received in the schools of Columbia and Winnsboro. At the latter place he attended the famous old Mt. Zion Institute, where so many prominent men of the Palmetto State have been trained. Dr. Taylor then entered South Carolina College in 1851, but was forced to leave on account of sickness after six months of work. He was determined upon an education and reentered, in the fall of 1853, and was graduated in the class of 1855 with honors. Dr. Taylor went to the South Carolina Medical College in 1856, 1857 and 1858, and during that time took a course of summer lectures in Philadelphia at the University of Pennsylvania. He was graduated at Charleston in 1858 and then read medicine with the Drs. R. W. Gibbes—father and son—and began work which he carried on for almost half a century. He was Surgeon of the Second South Carolina Cavalry, C. S. A., later Medical Director of the Cavalry Corps of the Army of Northern Virginia with the rank of Colonel. He was President of the South Carolina Medical Society. When the Columbia Hospital was built Dr. Taylor became the visiting physician, which place he filled devotedly until his death, and he was also the visiting physician to the hospital of the South Carolina College for many years. He was elected a member of the Board of Regents of the State Hospital for the Insane in 1878 and served continuously until the day of his death, and was also Chairman of the Board for many years.

At various times during his life Dr. Taylor has been surgeon of the old Phenix Hook and Ladder Company, the Atlantic Coast Line, the Southern Cotton Oil Company, the Southern Railway, Medical Referee for the Mutual Life Insurance Company, Medical Examiner for the New York Life, the Cotton States and the Granger Life and Health Insurance Companies for the State of South Carolina, and was perhaps, at the time of his death, the oldest and most distinguished member of the medical profession in the Carolinas.

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